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THE PRACTITIONER:

111
A Monthly Journal

OF

THERAPEUTICS.

EDITED BY

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THE PRACTITIONER.

JULY, 1872.

Original Communications.

CLINICAL LECTURE ON THREE PERIODS OF A CASE OF SYMPATHETIC IRRITATION OF THE EYE.

BY ROBERT BRUDENELL CARTER, F.R.C.S.,

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Ophthalmic Hospital.*

December 28th.—The patient before us, gentlemen, presents a somewhat unusual complication of conditions, the character and tendencies of which it would not be difficult to overlook or to mistake. She is a person of spare habit and feeble aspect, with a patch of dense white opacity in the left cornea, and with manifest paralysis of the portio dura on the right side. She is thirty-eight years of age, and tells us that she has three children, and has been three years a widow, supporting herself and two of the children by her needle and by working at a mangle. Five weeks ago she had a "fit," and recovered from it with the right half of the face paralysed as you now see it. The leucoma of the left eye dates from the time when she was four years old, and is the result of an abscess or ulcer of the cornea following measles. With this eye she has never had useful vision; and she now complains that the other, the sound eye, has been lately failing. She says that, after using it for a short time, upon any work requiring application, its vision

suddenly becomes dim, and does not return until after a period of rest.

Now, something similar to this is frequently observed as the result of a peculiar conformation of the eyeball, and is then readily relieved by spectacles. Persons in whom the antero-posterior axis of the eyeball is shorter than the focal length of the eye, are compelled to correct this disproportion by energetic contraction of the ciliary muscle, the muscle of accommodation, by which the focal length of the crystalline lens is diminished; and, when this muscle is relaxed by fatigue, they lose definition of near objects, or even sometimes of distant ones. Such persons are said to have hypermetropic eyes; and they are very numerous. We constantly have patients here whose chief complaint it is that they can read or sew for a little time, and then everything becomes dim. They rest for a moment or two, close the eyelids firmly, or compress the eyeballs with the hand with a very characteristic gesture, and then they are able to go on again for a time. By giving them convex spectacles of appropriate strength, the work of the muscle of accommodation is done for them by external help, and the eyes are relieved from fatigue. In order to determine the presence and the degree of hypermetropia we paralyse the ciliary muscles for a time by the application of atropine, and then test the vision either by lenses or by the ophthalmoscope. I therefore applied atropine to both eyes of our present patient, and awaited the result.

After the lapse of half an hour, we found the right pupil fully dilated. The left pupil was well dilated on the inner side, but its lower and external third was shown to be adherent to the corneal cicatrix. The patient made no complaint of increased dimness of vision with the right eye, as she certainly would have done had she been hypermetropic; and the first attempt to see its fundus with the ophthalmoscope, in the direct image, showed that she was, in truth, a little myopic—the axis of the eyeball being longer than its focal length, and the retina just beyond the range of perfectly clear vision. It was at once rendered sure that the temporary obscurations of sight could not depend upon fatigue of accommodation; because the accommodative effort required from such an eye would be too small to occasion fatigue. On further ophthalmoscopic investigation,

using first the inverted image, and then the direct image with the aid of a weak concave lens to neutralise the myopia, it became apparent that the intra-ocular circulation was not of normal freedom. The arteries are of less than normal calibre, and the veins, although distended upon the retina, appear in the inverted image to taper off almost to points upon the centre of the optic disc. This appearance is an optical illusion, and depends upon the disc being somewhat cupped, or hollowed out into a sort of funnel-shaped excavation, by increased intra-ocular pressure. You know that the vessels and other details of the fundus of a myopic eye look smaller in the inverted image than those of a natural eye, because they are further away; and the vessels in our patient's eye look smaller on the disc than on the retina for the same reason, and appear to taper to points because they recede gradually, and the more distant portions appear the smallest. The proof of this is that we can vary the aspect of affairs by using the direct image with a neutralising concave lens, and can then see the veins in their full calibre upon the disc. We next place a finger lightly upon the upper lid during the ophthalmoscopic examination, and a scarcely perceptible pressure occasions venous pulsation; the veins becoming full and empty alternately—synchronously with the radial pulse and its intervals. With a sufficient magnifying power we may see that, at the acme of the pulse-wave, the blood in the veins recedes from the larger into the smaller branches, leaving the portions on the disc momentarily empty, and that it returns as the pulse-wave slackens. The meaning of this so-called venous pulse is, that the eye is over-full, and that the arterial blood can only enter by pushing the venous blood out of its way.

We are led by this investigation to feel the degree of hardness of the eyeball, and we find it to be manifestly increased, notwithstanding the enlargement of the cavity produced by the cupping of the nerve-disc. To refine somewhat on Mr. Bowman's nomenclature, we may say there is $T + \frac{1}{2}$.

It is quite plain that the state of circulation here existing must tend to produce blood stasis in the capillaries between the artery and the vein of the retina, and to impair the nutrition of the parts supplied by them. If the arterial blood enters the eye,

with difficulty, and only by emptying the widest portions of the veins at the expense of the narrower portions further back, the blood in the veins must yet act as a sort of elastic buffer against the pulse-wave, tending gradually to bring it to rest, and to prevent the blood-current from reaching the capillaries in normal force. We should hence expect to find that lowering or loss of the function of the peripheral portions of the retina which is called "contraction of the field of vision." I have measured the field by means of Professor Förster's perimeter, and the resulting diagram confirms this expectation.

We have, so far, the physical type of an early stage of the condition called "glaucoma." If the existing tension were to increase slowly, we should find the excavation of the nerve-disc become more complete, and the cavity would acquire perpendicular instead of sloping walls. The nervous pulse, now requiring the slightest finger pressure for its production, would become an established phenomenon. The vessels, instead of appearing gradually smaller in their outward course, would hook round the abrupt margin of the cup, and would be faintly seen upon its floor. The field of vision would become smaller and smaller, and at last total blindness would supervene. If the increase of tension were more rapid, some pain would be produced. If the increase occurred in a succession of sudden small increments, each of these would be attended with obscuration or clouding of the field of vision, passing away in some degree as the capsule of the eye accommodated itself to the pressure, but each leaving the sight permanently worse than before. If it occurred in a sudden large increment, the disease would be called acute or fulminating glaucoma; and vision might be destroyed in a few hours, if not saved by iridectomy. Every type of glaucoma is produced by variations in the rate and manner of increase of the tension; but all the types are alike in this, that they tend, sooner or later, to the extinction of sight.

Our patient, although undeniably on the threshold of this danger, has not reached a period of life at which glaucoma is common as a condition independent of any apparent exciting cause; and we therefore inquire how the increase of tension has been produced. In relation to this question we obtain valuable guidance from the occasional and temporary loss of sight already

mentioned, and which we have seen cannot be ascribed to failure of accommodation.

In ordinary continuous vision with two eyes, there are many reasons for believing that, although they are always acting in concert, yet that they relieve each other by being, so to speak, active or essential, and passive or complementary by turns. If we fix both eyes on a small distant point of light, such as a distinctly visible and tolerably isolated star, we can keep it in view as long as we please, or until the muscles are tired of maintaining the necessary direction of the gaze. But if we close one eye and fix the star with the other, it will in a short time vanish; after a while we recover it, and if we again fix it with the same eye, it will vanish more quickly than before. The explanation seems to be, that the retina of the one eye becomes functionally exhausted at the spot on which the image of the star falls, and thus insensitive to the image until restored by a period of rest. When both eyes are concerned, each takes up the active seeing in its turn, and so relieves its fellow:

The loss of the star by the single eye is called "interruption of vision," and a similar phenomenon is common to all one-eyed people who have to gaze fixedly at any single point or small surface. Generally, however, the interruptions chime in with natural intermissions of effort, and are hardly noticed as sources of inconvenience. When they become so, when "interruption" noticeably interferes with the business of life, it is often an early and very important indication that the affected eye is sympathetically irritated by its fellow.

In the case before us we have to distinguish "interruption of vision" from the temporary obscuration which I have spoken of as incidental to glaucoma when increasing by sudden small increments of pressure. It is easy to do this, because the glaucomatous obscurations cover the whole field of vision, and are most dense at its periphery. The "interruptions" affect only the direct vision, and leave the margin of the field as before. The patient ceases to see what she was looking directly at, but sees all around, and has no sensation of general loss of light. Such, you will find, is the description given in the case before us, and it is sufficient at once to direct our attention to the other eye.

We find here, as I have said, a dense, white, sharply defined, corneal opacity. It is elliptical in shape, its centre situated a little below and to the outer side of the centre of the cornea, and its major axis extending from above downwards and inwards. It nowhere reaches to the margin of the cornea, and on its inner aspect it does not conceal the margin of the moderately dilated pupil; so that there is a considerable pupillary opening in this direction when atropine has been employed. Through this opening the patient has vision of large objects: but the leucoma covers the undilated pupil almost entirely, and has caused the eye to be for more than thirty years disused. From this disuse (as in the case of squinting eyes) the functional activity of the retina is very low, and the eye, unused to fixation, deviates somewhat in an outward direction. The form of the cornea is unaltered by the cicatrix, but a considerable portion of the pupillary margin is firmly adherent to it.

On a review of all these circumstances, I came to the conclusion that the adhesion of the left pupil to the corneal cicatrix had become a source of irritation of the eye, and that this irritation was affecting also its otherwise over-worked fellow, and had produced the over-secretion by which the eyeball was distended, as well as the very noticeable interruption of vision of which the patient has complained. I shall therefore, in the first place, make an iridectomy in the left eye downwards and inwards, selecting that position because it is the best for optical purposes, and because a pupil so placed may possibly bring about some improvement of sight. At the same time, after excising the selected portion of iris, I shall introduce a hook into the anterior chamber, and detach the adhesion to which I have called your attention. When the eye has recovered from this operation, I shall perform an iridectomy upwards, in the ordinary manner, upon the right eye, for the purpose of relieving its increased tension. The artificial pupil so made will lie beneath the shelter of the upper lid, and will neither be conspicuous nor a source of impairment to vision. The good effect of these operations will be only gradually obtained; and, as soon as they are performed, the patient must be placed under the care of a physician for the treatment of the facial paralysis—a matter to which it would not fall within my province to direct your attention.

As far as the eyes are concerned, the chief practical lesson to be drawn from this case is the danger of leaving adhesions of the iris, whether they are posterior—that is, adhesions to the capsule of the lens, resulting from iritis—or anterior,—adhesions to the cornea, resulting from wounds or ulcerations. In either case the continual check produced by the adhesion upon the functional movements of the pupil, upon its free dilatation and contraction under variations of light, is almost certain to become a source of future mischief. It constantly renders iritis a recurrent and formidable malady. It sometimes produces general internal ophthalmia of the most destructive kind; and when the adhesion is to the cornea, as in the case before us, the vessels of the iris feed the corneal cicatrix, and prevent it from being absorbed. I have little doubt that in this woman the cornea would have cleared itself, or at least would have presented only a slight haze or nebula, if the cicatrix had not been nourished by this undue vascular supply. Never consider a patient cured, or suffer him to pass from under your observation without warning, so long as any portion of his iris is abnormally adherent either to the capsule of the lens or to the cornea.

January 27th.—I present to you again to-day the patient on whose case I made some observations four weeks ago. In the interval, the operations which I then proposed have been carried out. The right eye has been iridectomised directly upwards, the left downwards and inwards, and the adhesion of the iris detached. You may observe that there is now a distinct interval, filled by aqueous humour, between the plane of the iris and the inner surface of the cornea, and that the remaining portion of the pupillary margin is free to move as circumstances may require. In the performance of this operation I employed a hook which is a somewhat modified form of one first suggested for the purpose by Mr. Streetfeild. It is a sort of little steel spatula, round and blunt, with a rather large deep notch cut in one side, and the inner edge of this notch sharpened. The spatula is set in an ivory handle like that of a cataract knife, and is itself bent on the flat at an angle of 120° , so that it can be easily introduced at any part of the cornea. After the iridectomy, this little spatula was passed into the eye, was made to glide between the cornea and the iris, with its notch towards the adhesion, until

the latter was hooked in the notch. If the notch had been blunt and I had pulled at the adhesion, I might possibly have separated it; but I might just as possibly have torn the iris from its attachment at its outer margin. From this risk I am saved by the cutting edge of the notch, which divides the part against which it is made to press. The eyes are still a little vascular and irritable; and it would be premature to make any trials of sight at present. The patient will now go under treatment for her facial paralysis, and in due time we shall see her again.

June 24th.—Our old acquaintance comes before us once more, this time with only one of her maladies conspicuous, and very anxious to be relieved of that. With her right eye she reads brilliant type easily. Its tension is quite normal, and if you examine it with the ophthalmoscope you will no longer discover the apparent tapering of the vessels, nor will you bring out a nervous pulse without strong pressure. She still complains of some degree of interruption of vision; but as this is in one sense a physiological phenomenon, and as her attention has once been directed to its occurrence, it is not likely that she will ever altogether lose it. The eye appears, as far as we can see at present, to be in no danger of any further changes.

In the left eye the vision has improved. The patient is able with it to read No. 19 of Jaeger's test types; and it would not fail to improve further if she were able to devote time to its cultivation by regular exercise. Its appearance is also altered for the better by the absence of the staring cicatrix, which I have tattooed with Indian ink, so as to render it a black spot instead of a white one, and thus scarcely more conspicuous than the natural pupil. This operation of tattooing was first suggested a few months ago by Dr. de Wecker, of Paris, and has since been largely practised. It may be performed with any kind of needle, and the one which I use myself is a kind of vaccinator, carrying its Indian ink in its groove. When the pigment is of the best quality, scarcely any irritation is excited, and the eye hardly needs to be tied up after the punctures.

When the patient left us she passed for a time under the care of Dr. Anstie, who saw reason, on inquiring into her case, to give her large doses of iodide of potassium. To this, and to the employment of electricity, the palsy of the portio dura has

gradually yielded, while her health and general condition have greatly improved. Her remaining trouble is the slight divergent squint of the left eye, to which I called your attention when we first saw her, as a mere wandering outward of the organ from disuse. It is not important; but she is sufficiently proud of her tattooed leucoma to consider the squint a serious defect—a sort of ragged button on the court dress of her improved condition. This being so, I propose shortly to divide the external rectus muscle, and, if necessary, to bring forward the attachment of the relaxed internal rectus; and by these means to restore her eyes to their original harmony of movement and of position.

TWO CASES OF RHEUMATIC HYPERPYREXIA: ONE TREATED WITH THE COLD PACK.

BY CHARLES KELLY, M.D.,

*Assistant Physician to King's College Hospital, and Physician to the Evelina
Hospital.*

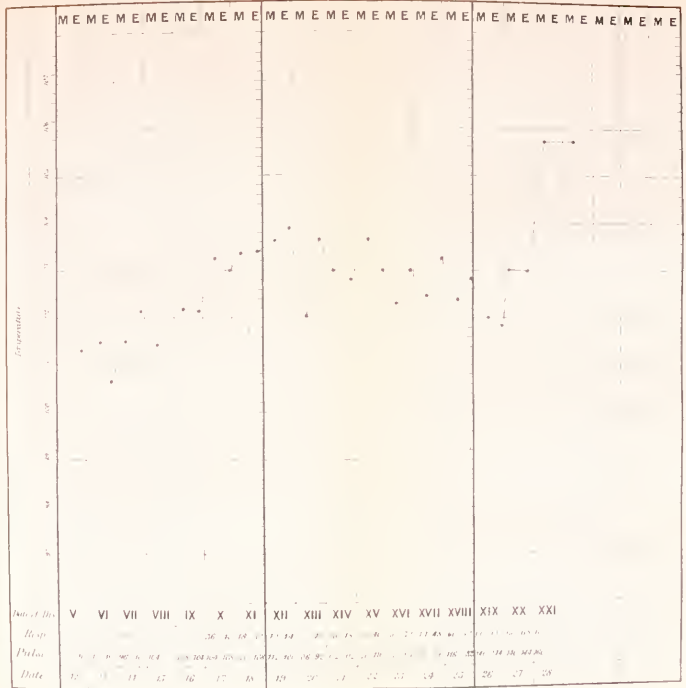
THE accompanying cases present several points of interest: both suffered from their first attack of rheumatic fever; both had similar symptoms, and in both the pericardium was inflamed. The man seemed to be going on very well for a time, but then his temperature went up, and he died; the woman, who was equally ill, and in whom the temperature was still higher, was cooled down more than six and a half degrees in four and a half hours, and made an excellent recovery.

In the latter case the prostration was so great that it seemed desirable to pack her in cold sheets rather than put her into a cold bath; and from one or two other cases in which I have tried the cold bath and packing, I am inclined to think that the latter method is most simple and advantageous.

CASE I.—William P., aged twenty-one years, was admitted into King's College Hospital under my care during the absence of my colleague, Dr. Garrod, on March 12, 1872.

He was a well-nourished young man, and up to the present illness he had always had very good health. Formerly he had been employed as a steward on board a yacht, but latterly he has been a footman; he had always lived well, and was known as a very temperate and steady servant.

On March 7th he found that both his ankles were stiff and rather painful, and this increased towards evening, so that at bedtime he could hardly stand, and the ankles were swollen. At the same time he was thirsty and sweated a good deal, and



March 1872

his appetite was much impaired. He continued getting worse; and although his ankles became easier, yet the knees and hip became affected, so that any movement of his lower extremities gave him great pain. On admission he complained of pain in the ankles, knees, and right hip, also in both shoulders, and most severely in the metacarpo-phalangeal joint of each thumb, and in the left wrist; all these joints were swollen and red. The tongue was covered with a white moist fur, and the skin was moist, but not freely perspiring. The heart sounds were healthy, but the apex was rather more external than usual; there was no pain or sense of constriction across the sternum, and the breathing was quiet. The temperature ranged between 101° and 102° Fahr., and the pulse was 96.

The case, in fact, was one of rheumatic fever of moderate severity. Blisters were applied to the painful joints by brushing lig. epispast. over the adjacent skin, and this gave him much relief; he was put on a diet consisting of milk and beef-tea, and four ounces of brandy every twenty-four hours.

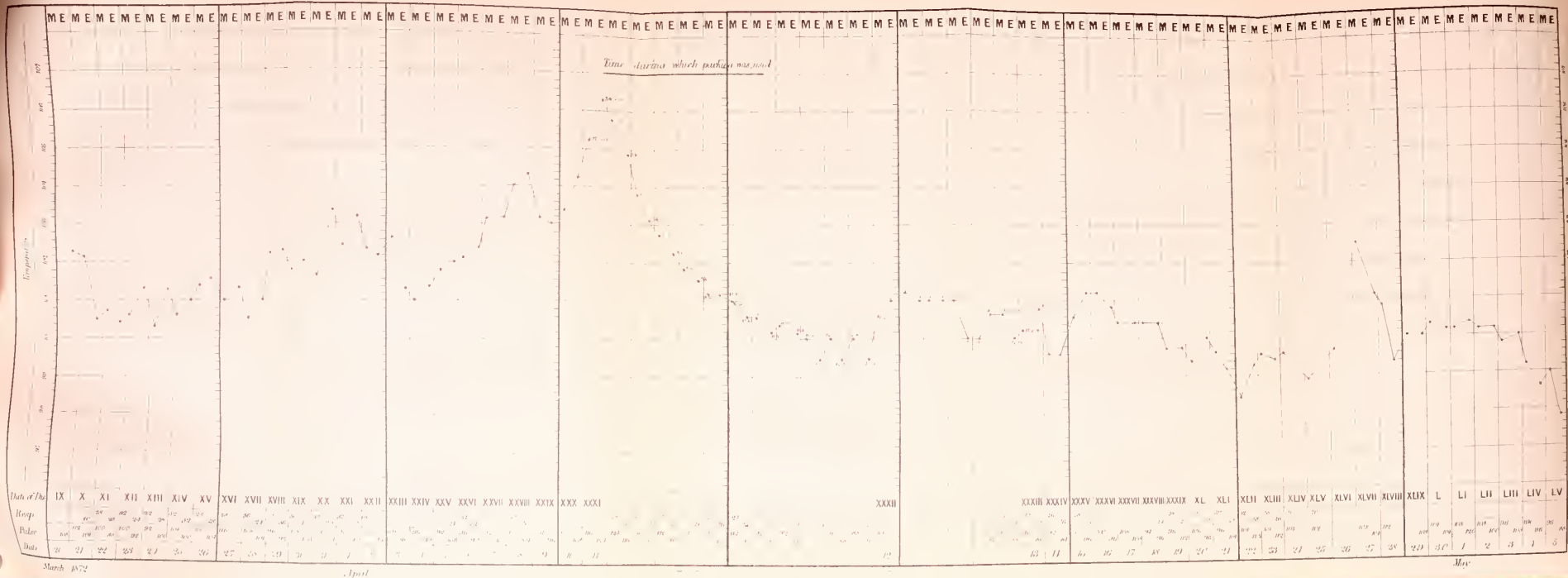
On March 15th, he seemed in much the same state. The pain had ceased in some of the joints, but had spread to others; he lay on his back in a very prostrate state, and perspired very freely; the tongue was thickly coated, but he drank plenty of milk. Still no abnormal sounds could be heard, either over the heart or lungs. The urine was rather high-coloured, and free from albumen. At night he was very restless, and slept but little, but during the day he was quite sensible, and expressed himself as being freer from pain. The affected joints were wrapped up in cotton-wool, and no medicine was given except a little camphor-water three times a day.

The patient complained on the 17th of great pain across the front of the chest, and chiefly over the region of the heart, and on auscultation a loud to-and-fro friction sound could be heard. He lay on his back and breathed quickly, but could not expand his chest properly in consequence of the pain. Four leeches were applied over the heart, and their application gave him great relief; and he was still more relieved on the renewed bleeding which took place to a slight extent in the evening from the bites. At this time all the pain in the joints had ceased, but he now complained of difficulty in swallowing, and this symptom

gave him up to his death much discomfort; it seemed due to the pressure of the effused fluid in the pericardial sac on the œsophagus behind. Copious crops of sudamina appeared on the body, and chiefly about the chest and shoulders. The temperature had been gradually rising since admission, and on the tenth day of illness (March 17), when the rub first appeared, it was 103° Fahr.; the pulse, too, was more frequent, and the respirations between thirty-six and forty per minute. The tongue was tremulous and thickly coated with a white fur; the face was pale and anxious; very little sleep was obtained; the mind was clear, but the man lay in a state of great prostration. Although swallowing was painful when the food passed down the middle third of the œsophagus, yet he took about three pints of milk per diem; the brandy was now increased to eight ounces in the twenty-four hours.

On March 20th the temperature was still higher, and no improvement had taken place. Free from pain in all the joints, the man could only swallow with difficulty, and he suffered much distress from the pain in the chest; the pulse was very feeble, the countenance more anxious, and no sleep could be obtained at night. The pericardial rub could still be heard, and the cardiac dulness extended up to the second rib above, while laterally it extended from the right edge of the sternum across to the left mammary line. Considerable dyspnoea now came on, and to relieve it a mixture containing carbonate of ammonia and nitric ether was given every four hours, but without any marked benefit. In two days' time he seemed a little easier, and the breathing was less difficult; at night he was delirious, and had been so about a week, yet during the day he was sensible and conscious.

March 27th.—For the last five days very little change had taken place, but the prostration was not greater, and he still took plenty of milk. The tongue was very tremulous, but rather cleaner at the tip and edges; the pulse and respirations were as quick as before, but the temperature had begun to fall slightly: the cardiac dulness had not increased, but the to-and-fro sound had disappeared, and now a pleuritic rub could be heard in the left infra-axillary region; delirium was constant at night, and no sleep could be obtained. In the evening he was worse;



the respirations were more frequent, and the pulse feeble and rapid; the temperature was 103° Fahr. The worst symptoms were his great prostration and dyspnoea. The brandy was increased to ten ounces a day, and hot poultices were applied, during his illness, to the front of his chest.

On the morning of March 28th his temperature rose rather suddenly to 105.8° Fahr., and kept up to that point until noon, when the man died, on the twenty-first day of illness. At the autopsy, twenty-six hours after death, the pericardial sac was found to be distended with twenty-two ounces of dark-coloured serum, while both surfaces of the pericardium were coated with recent lymph, and in parts pretty firm adhesions had taken place; the valves and wall of the heart were healthy. The cardiac area was so much increased that the lungs were much pushed away; the left lower lobe was collapsed, and also the upper lobe to a partial extent; the corresponding parts of the right lung were similarly affected. The dyspnoea was therefore due to the small amount of breathing surface offered by the compressed lungs, and the difficulty of swallowing was caused by the pressure of the distended sac upon the œsophagus behind.

CASE II.—Maria M., aged thirty-six years, was admitted under my care on March 20, 1872, in the Twining Ward. She was a fellow-servant in the same house with William P. (Case I.), and the only cause that she could assign for her present attack of rheumatic fever was the fact that the room in which she slept had been lately re-papered and whitewashed, and from that cause it was damp. Previous to this attack she had never had any serious illness, and, as in the preceding case, this was the first attack of rheumatic fever. The illness began on March 12th with shivering and pain in the limbs; this went on until the 15th, when she became worse: the pain in her joints increased; she was thirsty, and lost her appetite, and was obliged to leave off work: there was no rash nor sore throat. On the 17th her elbows and shoulders were first affected, the pain still continuing in the lower extremities where it commenced. She became very feverish and delirious at night, so that her fellow-servants could not keep her in bed. On admission she seemed a fairly nourished woman, with slow, rather incoherent

speech, and a flushed face: she had pain in the hands and wrists, and to a less extent in the ankles and knees. During the day she was quite conscious, but at night she was very delirious; the face was flushed, tongue slightly furred, sweating at times profusely, and very thirsty. The cardiac dulness was normal, and no bruit or friction sound could be heard.

On March 23rd, the twelfth day of illness, she seemed better. The ankles, elbows, and shoulders, which had been swollen and painful, were now easier, and she hardly complained of her joints at all. She lay, however, in a very prostrate condition, and the breathing was somewhat hurried; the cardiac dulness extended up to the second intercostal space, and the apex of the heart was raised slightly; a rough sound could be heard with the systole at the base of the heart; there was no marked pain across the cardiac region, but the breathing was quick and shallow. At first, camphor-water was given three times a day, and twenty grains of chloral at bedtime, but little benefit seemed to proceed from its use, as she was very restless and delirious at night. Up to March 29th very little change had taken place: the pain in the joints was gone, but the dulness extended as high as the second rib on the left side; copious crops of sudamina appeared all over the body, and chiefly on the chest; she took plenty of milk, but the prostration was very great, and at night she was still delirious; the temperature ranged between 100°·5 Fahr. and 102°·5 Fahr., without there being much variation from day to day. On the 29th, five grains of quinine were given every six hours, in a mixture containing some bicarbonate of potash: no effect was produced on the temperature, which still continued to rise slowly; no improvement took place in her general condition, for she still remained in a semi-stupid state, and lay on her back unable to move herself in bed; she became in two days very deaf, and remained more or less so until the middle of April. On the night of April 1st, twenty minims of liq. morph. acetat. were given instead of the usual dose of chloral, and on the evening of the 3rd five grains of Dover's powder were given. By this means the patient obtained occasional snatches of sleep. At this time no rub could be heard, nor were the heart-sounds by any means distinct; the dulness increased and extended above

to the first left intercostal space, and laterally from the right edge of the sternum into the left axillary region. On the 4th of April the temperature was lower, and she passed a better night; but from that time up to April 11th the temperature gradually rose; she seemed more depressed and prostrate, and the delirium increased at night, although she had a sedative at bedtime. Quinine was still being given with no apparent benefit, and she was taking a milk diet with four ounces of brandy a day at first; on March 26th and up to April 11th she was taking six ounces of brandy in twenty-four hours. On the morning of April 11th the temperature was about 105° Fahr., and at half-past seven in the evening it had further risen to $106^{\circ}2$ Fahr.; at that time she lay in a state of extreme prostration, and was also very deaf. There was no change in the physical signs, but she complained now of severe headache over the occiput. Although not delirious, she seemed in a semi-conscious state, and talked in a slow, peculiar way. It was then resolved to try the effect of cold in lowering the temperature; and for the carrying out of the method, and for the careful notes and thermometric observations, I am much indebted to Mr. Warner, our house physician. A mackintosh being placed beneath the patient, sheets wrung out of ice-cold water were applied round the limbs and body; a pail was placed by the side of the bed, full of water, in which pieces of ice were melting, and the sheets were dipped in every few minutes. By this means a gradual and steady fall of temperature took place, and at midnight, after being packed for $4\frac{1}{2}$ hours, a descent of $6^{\circ}6$ Fahr. had taken place, and the patient was then at $99^{\circ}6$ Fahr. During the treatment she was sensible, and complained much of the intense cold. Every fifteen minutes the temperature was carefully noted, and a tea-spoonful of brandy was given every quarter of an hour, from 7.40 P.M. until 1.20 A.M.; then every ten minutes up to 9.15 A.M. (April 12th), then every quarter of an hour again up to 4.40 P.M.; from that time up to 10 o'clock in the evening, a tea-spoonful of brandy was given every twenty minutes; after that an interval of half-an-hour was allowed. While the cold was being applied she took two ounces of brandy, and from midnight until the afternoon of April 12th twelve ounces were taken; after that the quantity was diminished to six ounces a day. During the

packing the pulse and respirations gradually fell; but towards the end of the time the pulse became so feeble that it could not be continued. Enemata of beef-tea and egg were administered on the 12th. The urine was examined, and no albumen was found, nor was any present in the urine during her illness. A manifest improvement was found on the afternoon of the 12th: she was perfectly sensible; her temperature ranged between 100° Fahr. and 101° Fahr., and she seemed less prostrate; her appetite was also improved, and she slept a little. This improvement continued, so that on April 17th, six days after the packing, she expressed a desire to sit up; she felt much stronger, took plenty of nourishment, slept better, and her manner was much more cheerful; the deafness began to improve, and no medicine was given at all. The physical signs were the same as before. On April 19th she sat up for a short time, and felt much stronger; she was able to eat some solid food, and her breathing was quite tranquil; the dulness over the heart had diminished, and only reached up to the lower border of the second rib. At this time convalescence was retarded by an obstinate bed-sore, which, although of small size, yet prevented her sitting up. She gradually became convalescent until April 27th, when she did not feel so well, and had some pain in the left knee and hands. The temperature rose for a few hours, but the next day it descended, and the patient recovered without any complications. By the end of the first week in May she was quite free from pain or any cardiac distress or dyspnoea; she was able to sit up every day, and the bed-sore had nearly healed. On the 18th of May she was so far recovered as to be able to go into the country. The cardiac dulness was even then rather higher than usual, but no rub or bruit could be heard, and the patient expressed herself as feeling quite well.

The accompanying charts will show more clearly than a description, the daily changes in the temperature, pulse, and respiration in each case.

REMARKS ON INSTRUCTION FOR THE DEAF.

BY W. B. DALBY, F.R.C.S., M.B. CANTAB.,

Aural Surgeon to St. George's Hospital.

IN the course of discussion on any social reform, it is quite curious to notice how the progress of knowledge on the subject at times is retarded, not so much by interested partisans on one side or the other, as by inaccurate and loose statements made either from carelessness or imperfect information by the best intentioned and most disinterested of friendly critics. This has been very well instanced lately in the attempts which are now being made to introduce into this country a system of instruction for the deaf and dumb, whereby they are taught to read from the lips of others, and to use articulate speech; in fact, the same system which is universally employed in Germany and Holland.

Having convinced myself of the superiority of this method of teaching over any other, in a paper which I read at the last Social Science Congress at Leeds, reported in the *Times* of October 13th, 1871, I traced the history of lip-reading so far back as the year 700 A.D., and gave a short account of its subsequent practice in isolated instances, in Italy, Spain, and England, until its almost universal adoption in Germany and Holland. The method of teaching was then detailed, and it was shown how and why it was impossible the system could be combined with the one now employed in England, viz. talking on the fingers and by manual signs. It was further shown how all totally deaf children (except in the case of a cleft palate), who were not mentally deficient, could be taught, and that the course of education spread over a space of from seven to eight years (longer than on the English system); but that after that period, a child so taught would be able to understand what was

said to him by watching the lips of the speaker, to reply intelligibly in return, and would be possessed of a fair general education.

In a leading article in the *British Medical Journal* of Nov. 4, which gave a most fair notice of the subject, the system was inadvertently spoken of as "new." This brought an anonymous letter into the next week's journal, signed "Truth," the writer of which explained at length that it was by no means new; and, being strongly opposed to the system, he deprecated its general adoption, unless "we are content to produce a speaking automaton when we might draw forth a living soul;" which, though perhaps an impressive, is a somewhat vague way of putting the question. Again, the article pointed out most truly that it is impossible to combine successfully the two methods, and explained as follows: "So long as the children employ the finger alphabet, and their attention is thus withdrawn from the lip movement, they will not exercise the necessary perseverance to learn articulate speech." My own impression is, the correct explanation of this will be found to be, that if the young children have learned by finger talking to make their wants known, although their attention may be called to the lips, they will not possess enough reason to induce them to concentrate their attention and maintain it with sufficient perseverance to allow of the requisite exercise of the true muscular sense possessed by the lips, tongue, palate, &c.; for I believe it is owing to the high development of this, that children are enabled to imitate with such accuracy the movements which they are being taught. The letter of "Truth" concludes by remarking of the (so-called) German system: "Its proper position, therefore, in the system of instruction is not as a base or foundation, nor yet as the principal material in the superstructure, but rather as an adornment to certain portions of the building."

It is quite clear that if the upholders of Dactylology can induce attempts (which are sure to end in failure) at partial instead of complete change in the instruction of mutes, they will be successful in preventing the general adoption of the other method into England. For the same reason the teachers of Dactylology will, in refutation of the correctness of this view, always be ready to produce persons who can both speak well

articulately and talk on their fingers skilfully, representing them as rare examples who have been able in consequence of unexceptional mental capacity to acquire speech. So far, however, as I have been able to meet with them, upon a rigid investigation of the case, I have found that they have been originally taught to articulate and read from the lips, and that after they could do this, they have acquired the other mode of talking. This is of course quite a different case, and is not at all foreseen by the impetuous advocates of the German system, when they rashly challenge (as I have heard them do) the teachers of Dactylology to produce a totally deaf person who makes use of both methods for purposes of conversation.

On the 17th January an elaborate paper on the oral education of the deaf and dumb was read at the Society of Arts by S. Dasent, D.C.L., and it is reported in the *Times* of 19th January. After the paper there ensued a warm discussion between teachers on either side; the English teacher producing a decided impression when he narrated with effusion an instance of one of his little pupils being visibly engaged in prayer shortly before its death. This touching result of his education would perhaps have been more relevant to the point at issue if the main object in educating these children was to conduct them at a tender age to a happy termination of their unfortunate position. The really important question is, Can children who are dumb in consequence of deafness, either congenital or acquired in infancy, be so instructed in the course of from seven to eight years, as to articulate clearly and understand what is said to them? If this be so, there can be little doubt but that this is the best possible condition for them to be placed in. It is proved indisputably in several continental countries that this can be achieved, and lately in England there has been abundant evidence of it also. Until, however, more precision and accuracy is observed when speaking on the subject, this system will not become general in this country. For example: it has been stated over and over again that from seven to eight years is all the time required in teaching articulation and lip-reading until proficiency has been acquired. A few weeks ago, on finding that a child four years and three months old was so extremely and incurably deaf as to prevent its acquiring speech, I advised the parents to

employ the so-termed German method for its education, and to place it under the charge of a competent teacher so soon as it was of the proper age to commence with the course of instruction. I found it quite impossible to get a decided opinion from the teacher chosen as to when instruction should begin. "The sooner the better," he said. I reply, "At once, then; and the child will be able to talk well at between eleven and twelve years of age." But to this the teacher demurs, and says, "Not till he is between fourteen and fifteen years old." On close questioning he suggests five, and finally six years of age, as the best time at which to commence. In the face of this it is not to be wondered at that the writer, "Truth," before referred to, should remind us that "two preparatory years are passed in acquiring the elements of words, making a total of ten years." It would clearly have been absurd to attempt to teach a child at four years of age. In short, what I believe to be one of the most desirable changes in the instruction of the deaf in England, runs an eminent risk of being delayed by the very men who are laudably anxious to support it. I regard, however, the system as likely to be of immense service in the case of others, besides those who as infants are totally deaf. So little hearing may be left that the patients will never understand by means of hearing a spoken word, and yet just enough to give modulation and harmony to the voice, thus avoiding the harsh sound to speech which distinguishes mutes taught on the plan. Again, enough hearing may be present to permit of words being heard when spoken loudly close to the ear, and with these children this may be used to correct the pronunciation and thus shorten the course of instruction materially. The most noticeable good, however, arising from the system is shown where children who have spoken become totally deaf, from scarlet fever, inherited syphilis, and the like.

It is well known how in a few months they become dumb, but if they are at once taught to read from the lips—a matter of months, and not years—unlike as in teaching articulation to those who have never spoken, they will retain their speech, for their eyes will serve them for ears.

The totally deaf speaking adult may also with advantage be taught lip-reading, but for them it is a very tedious process.

WESTMINSTER HOSPITAL PAPERS.

XVI.

CASES OF OBSTRUCTED LABOUR IN WHICH THE CÆSAREAN SECTION WAS TWICE PERFORMED AND ONCE AVOIDED.

BY DR. FREDERIC BIRD.

PART II.

CASE II.—A. B., a pallid but not unhealthy-looking woman, was admitted into the obstetric ward, the subject of pregnancy complicated with uterine fibroid. The narration of her case ran thus:—Contemplating marriage in her thirty-eighth year, and aware of the existence of a uterine tumour, she sought the opinion of a physician, who, not realising the obstetric importance of the question, gave her permission to marry, and this erroneously deduced assent had this much justification—that so much of the uterus was involved in the tumour, so removed from digital detection its os, that impregnation might have seemed most improbable; but the ways of spermatozoa are beyond our ken. Pregnancy speedily followed marriage; in supposed security the time passed when the premature removal of the fœtus might have averted danger, and two or three weeks before the fulfilled term of utero-gestation she entered the hospital. Then an irregularly shaped tumour was found partially divided into two portions by a constricted boundary, which seemed to be formed by the projecting fibroid below, and

the dilated portion of the uterus containing the fœtus above; this corresponded to a line drawn from the right crista illi to the false ribs on the opposite side. The lower portion was very hard and unyielding, immobile, and announcing internal pressure on pelvic emergents by pains in the track of crural nerves, dysuria, and difficult defecation. (Edema, too, was adding its evidence of intra-pelvic pressure. Internal examination found a large hard fibrous growth stretching across the brim of the pelvis, with a depending portion in large measure filling up its cavity, quite immobile, and leaving little space for the nominal pelvic contents. Neither external cervix nor os could be felt, an extension of the vagina upwards and to the left of the tumour alone indicating their position. Externally, the upper part of the fibroid seemed as hard as the basic, and as immoveable; it filled the right iliac fossa, partly the left, bulging out the pubic region below and above, merging into that part of the uterus containing the fœtus, the distinction between the uterine portions affected by disease and gestation being marked by modulation of the former and planeness of the surface in the latter, which was marked also by softness and elasticity, and was the site of the fœtal sounds of circulation, both cardiac and placental being readily detected. There was no separability of these growths—the one belonged to the other. Nevertheless attempts were made to elevate the fibroid from its pelvic position; they entirely failed. Before long symptoms of coming labour set in, the pains occurring irregularly, though often severe. It was hoped, on no sufficient grounds, that some help might be tendered by nature—some softening, as has been said to have happened—some elevation of the tumour, as has happened; but none came. In their stead a rush of liquor amnii, and with it increased uterine action. Nothing remained to afford a chance for life-saving but the abdominal section. Before constitutional exhaustion set in—and it was threatening—the operation was performed. On opening the abdomen the tumour was found to occupy the parts already alluded to, but extending rather higher than had been opened. I therefore enlarged the incision upwards. There was little choice of site for uterine incision, and it was immediately followed by active hæmorrhage. The placenta was exposed, and hence its source. The placenta was at

once detached and removed, and, without separation from it, the child. The bleeding ceased, although the uterine wound remained patent; its lips were compressed, but the wedge-like action of the subjacent fibroid prevented the approximation of the lower portions. When it was evident that the bleeding did not recur, the abdominal wound was closed, cold-water dressing and bandage applied. The patient, recovering from the effects of chloroform, expressed herself as greatly relieved from suffering, and begged for her child. Her pulse was good—no sign of collapse. The child, a fairly developed girl, cried loudly on first exposure to the air, and is now well grown and strong, adding one more to the Cæsares and (she was delivered by the feet) Agrippæ too.

The night passed well; little sickness, and that from chloroform, occurred; food was taken willingly, and sleep came. The next morning was not discouraging, but before the day had closed symptoms of inflammation occurred, soon becoming marked by great and painful tympanitis; then vomiting, restlessness, and fast-failing pulse. As night advanced the distress became greater; remedies gave no relief; despite stimulants, the surface grew cold; respiration grew, from greater and painful abdominal distension, more rapid and more short; unconsciousness quickly came—and so she died.

No autopsy could be obtained, but it was sufficiently evident that the fibroid was seated in the posterior wall of the uterus, and steady increase of size had carried the upper and anterior part of that organ high above the pelvis; and although its limits were marked by a line in which the measurement was less, giving rise to an obvious depression, yet that it was inseparable from or sessile upon the uterus, and so could not have admitted of the treatment successfully adopted in the third Case, in which the presence of a peduncle allowed of change of position of the tumour.

CASE III.—Mrs. W., in her thirty-third year, of healthy family, and herself in good general health, was admitted into the obstetric ward with the following history:—Was married, and became, as she supposed, pregnant early in July 1870, the catamenial secretion having at that date been almost entirely absent. The fact that she had been severely stung by a

hornet induced doubts as to the cause of this arrest; but morning sickness occurring about three weeks afterwards, suggested a less accidental origin for its suspension. She continued to increase in size abdominally, and no other symptoms occurred until November 19, when she was attacked with illness, for which she sought the opinion of Mr. Ray, of Lowestoft, who thus writes:—"Was summoned to Mrs. W., and found her suffering from severe pain in the abdomen. I ascertained that she thought herself pregnant, and presented the enlargement indicative of the full term of pregnancy. On examining the body there was evident peritonitis, and two distinct tumours, one of which I thought to be ovarian, and fancied I could detect fluctuation, though indistinct. The other was of a harder character, not unlike the uterus in gestation, but not so large as one would expect at the time (she would be now five and a half months). The peritonitis yielded readily to treatment, and the abdomen became somewhat reduced in size."

The cause of the peritonitis is not clear, and Mr. Ray did not suggest any. It was, however, observed by that able practitioner, that two tumours existed, one hard and impacted in the pelvis, and to the pressure consequent upon which the local inflammation may be attributed.

In January, when she was sent up to me for opinion, I found the following evidences:—Abdomen greatly but unequally enlarged, having a mean girth of forty-two inches; the axis of the enlarged abdomen was a line passing from the superior spine of the right ilium to the centre of the left hypochondrium; marked bulging and prominence in the right iliac region, none in the left, but the hypogastrium very prominent; the fulness extended to the umbilicus, where a distinct swelling could be felt and almost seen, as if dividing the abdominal tumour into two parts; for above this depression the abdomen was again distended as much as below it; the right hypochondrium was but slightly felt. By percussion two distinct growths could be clearly made out, the lower one bounded below by the pelvic brim, and above by the depression already noticed and the umbilicus, the upper one by these below and the ribs above, under the margin of which on the left side it could be felt passing. Resonance on percussion

announced the presence of the intestines chiefly in the lumbar regions. By manipulation the lower tumour was found to be very hard, slightly nodulated, and immoveable; the upper tumour soft, elastic, and capable of slight movement from side to side. Corresponding to the depression and nearly to the umbilicus, could be felt a narrowing of the tumour as the fundus of the one joined the base of the other, and a thick, hard, and broad band of union seemed to exist, but it did not allow of any lateral movement of the upper tumour upon the lower. The lower tumour presented the signs of a fibroid, the upper of a sac containing a living foetus; its walls were very thin; the foetal movements often felt; the placental sound audible at the summit, that of the heart on its left and low down, corresponding externally with a spot four inches above the umbilicus on the axis previously described. Examined by the vagina, a large segment of the fibrous mass seen bulging over the pelvic brim was found in the true pelvis completely impacted in the right half, and nearly so in the left, excepting where the rectum passed down, and in the course of which, but carried more to the left side of the pelvic brim, the cervix uteri, small, flattened, and soft, could be with difficulty felt. No upward pressure had the slightest effect in moving the tumour: forcible pressure externally conveyed some slight impulse to the exploring finger; pressure on the upper tumour, none.

The conclusion arrived at was that pregnancy existed, and that it was complicated with a large fibroid mass; that the sac containing the foetus was either the upper portion of the uterus, which had become divided, hour-glass like, into two portions, the one involving the fibroid, the other the foetus; or that the foetus was extra-uterine, in a tubal nidus. To this latter the objection applied that the foetal life was active, and the period of gestation advanced. It was decided to send the patient into the hospital, as there seemed but little hope of delivery except by the abdominal section. So complete was the pelvic occupation, that the finger could not be carried beyond a limited space.

On admission into the obstetric ward, in March, the only noteworthy change externally was greater distension of the abdomen, especially in the upper tumour, where the foetal movements were more active and more strong, thus dispelling

the assumption of tubal gestation. Internally the hard tumour was as firmly impacted as before, but the os seemed to have been brought lower down by elongation of the cervix; the latter, excepting softness, was virgin-like in form; the yielding os allowed of the passage of a soft catheter to the extent of eight or nine inches obliquely upwards in the direction of the upper tumour. It passed, not curving around the hard growth below, but directly upward to the position of that contracted portion which has been spoken of as a band conjoining the upper part of the lower to the lower part of the upper tumour. It was now evident that the fibrous mass was in the ovarium, and the uterus was compressed and the pelvis filled by it. I decided to try to effect displacement of this growth from the pelvis, and to bring down the uterus. Forceful pressure internally and externally was tried from day to day, but with no marked result; but it seemed that the left pelvic portion of the tumour was rather less hard—more yielding to pressure in itself, though not in its position. Pressure of a more severe and sustained kind was now adopted. Manual pressure, carried to the utmost extent consistent with safety, was steadily applied, and at last some slight but marked elevation of the tumour was gained. A massive and hard graduated compress was then applied just above Poupart's ligament and beneath the prominent part of the tumour, a second less hard compress being applied to the fundus of the upper tumour; bandages were then bound over the lower compress first, and so fixed to the flexed leg of the patient that its extension could be made to exert very great and continued pressure. In a less severe manner the upper tumour was treated, the hope being that, the presence of a peduncle being proved, one tumour might be made to change places with the other.

After a few hours had passed it was necessary to give the patient relief, but examination internally gave reasons for encouragement. Pressure continued almost constantly for twelve hours more was still most useful, for now the pelvic extension of the tumour had sensibly lessened, and more of the uterus could be felt; but peritoneal inflammation threatened, the pulse ran high, and fever set in. All pressure was then stayed, and opium and allied remedies employed; the danger

passed over, and the pressura was re-applied. On the following day the tumour had ascended so much as to leave nearly half the pelvic cavity free. A sponge tent was next carried high up into the elongated cervix uteri, and after the lapse of six hours replaced by a larger one—this on the 15th of March. On the 16th, the report given by the obstetric assistant, Mr. Fosbroke, to whose watchfulness the patient was now consigned, was—

“Patient had slight but lingering pains after the application of the second tent; three others of increasing size were passed at intervals of six hours.

“17th.—Os uteri much enlarged, pains occasionally occurring.

“18th.—Active pains have set in—towards night became suddenly severe, and almost without intermission, and a living female child was in about three hours expelled. On the coming on of each pain the sustaining pressure under the tumour was increased to antagonise the downward voluntary pressure which now occurred. The placenta soon followed.

“19th.—Mother and child doing well; the ovarian fibroid has again fallen, and the uterus passed out of reach.

“From this date there was nothing but regular convalescence to note; an abundant supply of milk appeared on the fourth day after delivery, and the child is nursed by the mother.”

The successful issue of this case was not obtained without risk, but much risk was sanctioned by the gloomy alternative of abdominal section. The sustained pressure was very severe, the manual efforts at displacement more so, and I was not surprised to find premonitory signs of inflammation occurring; and it was possibly due to the adhesions consequent upon a former attack of peritonitis that the fixity of the impacted tumour was enhanced. Earlier interference would have been useless, not only because the diagnosis became more certain with delay, but because the hope was added that some favourable change in a portion of the pelvic passage might be effected by the greater development of the uterus. In some degree this seems to have happened, for a greater portion of the uterine cervix descending, proves that some slight space had been gained. The applied part of the tumour also seems to have undergone some slight change, and had become somewhat pliant.

Longer delay could have led to no better conditions, for the

more rapidly augmenting size of the foetus would have more than outweighed any little increase in space that might have resulted, and this little would have been probably destroyed by the greater acquired density and contraction of the peritoneal adhesions.

Between this and the preceding case many points of similarity exist—fibrous tumour in both cases ; pregnancy in both—the one to die, the other to live ; and the only difference between them being a small layer of tissue forming a peduncle, an inch or so of fibre holding the doubtful balance—life or death.

XVII.

THE PHYSIOLOGICAL ACTION OF ALCOHOL: AN ANSWER TO DR. SUBBOTIN.

BY A. DUPRÉ, PH.D.,

Lecturer on Chemistry.

AN important paper under the above heading has been published by Dr. Victor Subbotin in the "*Zeitschrift für Biologie*," Heft iv. 1871. In this paper a number of experiments on the elimination of alcohol are described, on the strength of which Dr. Subbotin arrives at the conclusion that the total elimination theory may be correct, although he himself admits that the direct experimental evidence is very far from proving it to be so. As these experiments have evidently been performed with great care, and, when rightly considered, prove the total elimination theory to be incorrect, even under circumstances highly favourable to excessive elimination, it may be well in the present stage of the controversy, and before the ghost of elimination is raised once more, to point out what is the real significance of this research. In doing so we leave aside the question as to the extremely injudicious choice made in regard to the animal experimented on. Before, however, entering into this consideration, it may be well clearly to state what it is that is contended for on either side.

The adherents, then, of Messrs. Perrin and Lallemand contend that all the alcohol, or at least practically all, is eliminated again unaltered; that consequently alcohol cannot perform any useful function in the animal organism. The opponents of this theory in this country, chiefly represented by Drs. Anstie, Thudichum, and the author, on the other hand, contend that if moderate doses of alcohol only are taken, the amount eliminated is a minute fraction only of the quantity taken; that alcohol therefore is destroyed, oxidised, in the system, and must of necessity supply a certain amount of heat or energy. It is only when very large doses of alcohol are given, doses which produce extreme narcotic effects, that elimination takes place to any considerable extent.¹ This last point is all that Dr. Subbotin's experiments show, and this, as just stated, has not been disputed, and is at the same time entirely beside the real and only important question, viz. what becomes of the alcohol taken in moderate quantities.

The following are the experiments:—

In three cases the alcohol—2·3, 3·45, and 3·45 grammes of absolute alcohol respectively, diluted to a strength of 20 per cent.—was injected into the œsophagus of a rabbit, which was then at once placed under the bell-jar of a small apparatus constructed after the model of Professor Pettenkofer's larger chamber. For the purposes of injection and retaining the alcohol in the stomach, the œsophagus was laid open, and then closed again securely by a ligature. The air carried through the apparatus, about 200 litres, 7·34 cubic feet per hour, was passed through a series of vessels for the purpose of absorbing the alcohol, which latter was finally estimated by conversion into acetic acid, &c., according to the method first described by the author in 1867. In

¹ In all experiments of Dr. Anstie and the author upon the effects of doses which were moderately intoxicating, the amount of elimination by the kidneys, though considerable relatively to that which is produced by non-narcotic doses, was absolutely very small. We never, in the ordinarily drunken human subject, observed an elimination of as much as 1 per cent. of the amount taken in the 24 hours. It is only in cases of rapidly fatal alcoholic poisoning (such as have been reported by Dr. Bathurst Woodman, and of which Dr. Anstie informs me that he has also tested one case) that quantities at all resembling those found by Dr. Subbotin have been proved to escape. Dr. Subbotin's rabbits are comparable only to human beings who have drunk off half a pint or a pint of whiskey at once, for a wager.

a control experiment, in which a known amount of alcohol was slowly evaporated in the apparatus, 73 per cent. of the alcohol so evaporated was recovered in the form of acetic acid.

The rabbits were kept under the bell-jar for five hours and a half, during which time the proportion of alcohol eliminated through skin, lungs, and kidneys amounted in the three cases to 2.3 per cent., 7 per cent., and 7.5 per cent. of the quantity injected. In the first of these experiments the condensation of the alcohol was less perfect than in the other two.

In the fourth experiment, 15 cub. cent. of spirit of 30 per cent., equal to 4.341 grammes of absolute alcohol, were injected as before, the weight of the rabbit this time being given as 1,840 grammes. The rabbit was at once put under the bell-jar and left there for five hours: the experiment was then interrupted for an hour and a quarter, after which the rabbit was again put into the apparatus and left there for five hours and a quarter. Total proportion eliminated during eleven hours and a half, the proportion eliminated during the interval of one hour and a quarter having been calculated and added to the amount found, equalled 12.6 per cent. of the alcohol injected.

In the fifth experiment, 4.341 grammes of alcohol, diluted as before, were injected into the stomach of a rabbit at 6.30 P.M., and the rabbit left at liberty for fourteen hours. At 8.15 the next morning it was placed under the bell-jar and left there till 12.15: the experiment was then interrupted till 3 P.M., from which time it was again carried on till 7.30 P.M.

Proportion of alcohol eliminated from 8.15 to 12.15 = 1.43 per cent. Proportion of alcohol eliminated from 12.15 to 3 calculated = 0.98 per cent. Proportion of alcohol eliminated from 3 to 7.30 = 1.06 per cent. Total proportion eliminated during eleven hours and a quarter, or from the 14th to the 25th after the alcohol had been injected, 3.47 per cent.

Assuming that in this case, as in the fourth experiment, 12.6 per cent. was eliminated during the first eleven hours and a half, this would give a total elimination of 16.07 per cent. during twenty-two hours and three-quarters; or, calculating for the entire period of twenty-five hours and a quarter, the proportion may be set down as 17.8 per cent. To this should be added the loss, as shown by control experiment, by which the actual elimination

during the twenty-five hours and a quarter is brought up to 24·4 per cent. of the amount of alcohol injected.

Now, it will at once be seen that in all these experiments excessive doses of alcohol were injected. In the fourth experiment, the only one in which the weight of the rabbit is given, the quantity injected amounted to 4·341 grammes of absolute alcohol, the rabbit weighing 1,840 grammes. This would give a proportionate dose of 165 grammes of absolute alcohol, or 330 grammes (11·6 ozs.) of proof spirit, equal to about 13 ozs. of ordinary strong brandy, for a moderate sized man (70 kilogrammes weight). If it be borne in mind that this enormous proportion was injected in a single dose in the course of a few seconds into a rabbit the œsophagus of which was first cut and then ligatured, it ceases to be matter of wonder that a notable proportion of alcohol was eliminated. Nay, the fact that even under these circumstances not more than 25 per cent. was found to be eliminated in the course of the first twenty-five hours must be taken as the strongest possible evidence of the remarkable power for oxidising alcohol possessed by the animal organism. Anyone unaccustomed to the use of brandy, who has ever experienced the effect produced by even 2 ozs. of brandy taken as a single dose, may easily picture to himself the alarming symptoms which would be produced by the injection into his stomach of 13 ozs. of brandy, not to speak of the effects of cutting open and then ligaturing his œsophagus.

But the fifth experiment furnishes, in addition, direct experimental evidence that a great proportion of the alcohol injected had been oxidised during the first fourteen hours. Thus calculating, from the fourth experiment, the proportion of alcohol eliminated during the first fourteen hours, adding loss as shown by control experiment, it will be found to amount to 21 per cent. of the alcohol injected, leaving 79 per cent. still in the rabbit, supposing the total elimination theory to be correct. The mean amount of alcohol contained in the rabbit during this period of fourteen hours was therefore 89·5 per cent. of the amount injected (100 per cent. in the beginning, 79 per cent. at the end; mean, 89·5 per cent.), with an elimination of 21 per cent., or very nearly a quarter of the amount of alcohol present. Calculating in like manner the alcohol eliminated during a second period of fourteen hours, we find it to be 6 per cent. of the amount

injected (the proportion actually found amounted to 2.49 per cent. for eight hours and a half) ; still leaving in the rabbit 73 per cent. of the original amount. The mean amount of alcohol which should therefore have been present in the rabbit during this second period of fourteen hours is thus 76 per cent. of the original quantity, the elimination amounting to 6 per cent. only, or less than one-twelfth of this amount. Surely nothing further is wanted to show the utter incorrectness of this theory, and to prove conclusively that a considerable proportion of the alcohol injected must have been oxidised in the course of the first fourteen hours, in spite of the extremely unfavourable circumstances.

Before leaving this part of the subject it may be well to point out one very serious omission in these experiments ; namely, the estimation of the amount of alcohol still left in the rabbits at the end of each experiment. The author, however, is convinced that the only satisfactory manner of examining this question is that adopted by him in his recent research on the elimination of alcohol ;¹ namely, to continue the regular alcohol diet long enough to make sure that the equilibrium between consumption and elimination has been established, and then to estimate the amount eliminated during (say) twenty-four hours. If the total elimination theory were correct, the amount daily eliminated should then equal the daily consumption ; whereas, as has been demonstrated by the author, the amount eliminated under these conditions is a minute fraction only of the amount consumed, at least when moderate doses of brandy only are taken.

Dr. Subbotin next enters into some considerations as to the value of alcohol as an article of food, and comes to the conclusion that it is no food at all, but only a stimulant and relish. This is not surprising, seeing that Dr. Subbotin believes in total elimination. According to Dr. Subbotin, nothing is a food which is not a normal constituent of the body, or at least can directly enter into the composition of a normal constituent. But this definition, as pointed out by Professor Pettenkofer in a note to Dr. Subbotin's paper, is quite inadmissible, and would exclude some of our most valuable articles of diet—as starch, for example. Any substance which is capable of assimilation and

¹ Proceedings of the Royal Society, vol. xx. p. 268 ; and *Practitioner*, Nos. xlv. and xlvi.

subsequent oxidation by and within the animal organism, and thereby contributes some of the heat or energy necessary to sustain life, is a true article of food, and from this point of view alcohol must undoubtedly be looked upon as such. Pettenkofer, however, in the same note expresses his opinion that the amount of alcohol usually taken is too small to contribute any appreciable proportion to the total energy required per day, and he also, in consequence, looks upon alcohol more as a stimulant and relish than a food. In coming to this conclusion, however, Pettenkofer seems greatly to have under-estimated the amount of energy derivable from alcohol. Let us see.

One gramme of alcohol by its oxidation evolves 7,184 units of heat, or an actual energy of 3,079 metrekilogrammes.

One gramme of lean beef, as oxidised in the body, evolves 1,428 units of heat, or an actual energy of 604 metrekilogrammes.

Weight for weight, alcohol evolves therefore about five times the amount of heat, or actual energy, that lean beef evolves.

Now, it has been calculated that 9·3 ozs. of lean beef will supply the energy necessary to sustain respiration and circulation in the body of an average man during twenty-four hours, and this energy may be therefore supplied by rather less than 2 ozs. of absolute alcohol. This amount of alcohol is contained in about 4 ozs. of brandy, 12 ozs. of sherry, one pint of a good natural wine (such as German or French wine), and in about two pints of average ale or porter. These quantities, as will be seen, are by no means excessive, and certainly are consumed daily by many. It is perfectly clear, therefore, that the alcohol consumed daily, even if in moderate doses, is capable of supplying no inconsiderable proportion of the energy necessary to sustain life. Of course I do not forget the fact that beef has *other* claims for consideration as a food: viz. as a nitrogenous tissue-former. But modern physiological doctrines give a very much more restricted value to nitrogenous tissue formation, as a part of ordinary nutrition, than was formerly accorded to it.

It has next been found that the consumption of alcohol diminishes the amount of carbonic anhydride exhaled and the amount of urea excreted. From this it has generally been inferred that the alcohol diminishes the amount of available energy; but this by no means follows.

Thus, one gramme of lean beef, as oxidised in the body, produces 0.497 gramme carbonic anhydride; whereas the amount of alcohol capable of evolving the same energy is 0.201 gramme, which by oxidation produces only 0.383 gramme carbonic anhydride, or considerably less for the same amount of energy evolved. Again, it seems well established that the quantity of nitrogen eliminated as urea is a measure rather of the amount of nitrogen contained in the food taken than of the amount of work performed; and hence a food which contains no nitrogen will of course diminish the amount of urea discharged.¹ In fact, on consideration, it will be found that the observed diminution of urea following the consumption of alcohol is strong evidence in favour of the assumption that alcohol is a food, and that to a certain extent—as far as the production of energy, for example, is concerned—it is really capable of taking the place of, and therefore protecting, the tissues of the body from the wear and tear of life.

XVIII.

A REMARKABLE CASE OF PLEURO-PNEUMONIA, ENDING FATALLY ON THE TWELFTH DAY AFTER DEFERVESCEENCE.

BY DR. ANSTIE.

IF it be desirable to record, in a journal devoted to therapeutics, instances of the complete and seemingly causeless failure of all remedies, the case which is now to be related deserves such

¹ Two things are very necessary to be remembered in regard to the question of the effect of alcohol upon the elimination of urea. If alcohol be given, in health, in addition to a food ration containing the usual quantity of nitrogen, there is no reason to expect any change in the quantity of urea eliminated. But if alcohol be given in circumstances of fever, when the usual supply of nitrogen cannot be taken, but when, nevertheless, the destruction of tissues causes a large discharge of urea, it will be found that the elimination of urea is reduced. See for instance the experiments made by Dr. Anstie and the author on a typhus patient in the Westminster Hospital. (*Lancet*, 1867, vol. ii.)

record. The one irreparable defect in the history is, that it was impossible to obtain a post-mortem examination: but this need not prevent our deriving valuable lessons from the case.

J. P., aged eighteen, a labouring man, concerning whom we could learn no history of any previous illnesses whatever, was admitted to Westminster Hospital under my care, May 20th. He was at that time in a rather remarkable state: although the right cheek was flushed and even felt somewhat hot to the touch, the general temperature was obviously sub-normal. By some accident no thermometer was at hand, but none was needed to convince me of this fact. The patient was vomiting frequently a thin greenish fluid, and had been doing so constantly for the last twenty-four hours: and his general condition was distinctly, though not profoundly, that of collapse.

The history which the patient gave us was very clear. He had been quite well up to Thursday, the 16th; about 4.30 on the afternoon of that day, while at work, he had a distinct shivering fit and a general feeling of illness, which caused him to go home. A little later he was attacked with sharp stabbing pain in the left side; and he became very ill, with vomiting, short breath, cough, and spitting (not bloody). Up to Sunday (fourth day) he suffered from general feelings of feverish heat: these then left him, and the vomiting increased to the degree in which we found him suffering from it on his admission.

On examination the skin generally felt cooler than normal, and even the tongue seemed somewhat cold; the pulse was 80, small and weak; patient seemed much prostrated. On examination of the chest there was nothing abnormal on the right side except puerile breathing, which was loudest behind. The left side gave a dull wooden percussion sound in front (as high as the fourth rib), and laterally up to the root of the posterior fold of the axilla: behind, there was marked dulness right up to the spine of the scapula, somewhat more wooden in the lower half of the chest. In the left chest, in front and laterally, no breath nor voice sounds could be heard, nor was there any vocal vibration over the area of most positive dulness. Behind, there was tubular breathing, seemingly close to the ear, from the top of the lung to the bottom, but becoming rather fainter in the latter situation. Diagnosis—left pleuro-pneumonia, fifth day:

Defervescence complete, vomiting and collapse from causes unknown—probably the extension of a catarrhal process to the stomach. As regards catarrh, it should be mentioned that there was a crop of herpetic vesicles at each angle of the mouth: these vesicles were in the stage of commencing desiccation.

If we were to sketch the subsequent progress of this case in the fewest words, we might say that the patient remained substantially in the same condition for ten days, only getting gradually weaker, and developing a tendency to delirium (especially towards night), and then died, still in the collapsed state.

The fluctuations of the case, as will be seen from the following record, were very slight.

The patient was admitted on the 20th of May. On the following day the thermometer verified the fact of absolute defervescence, marking indeed only $97^{\circ}6$ at 4 P.M. The spontaneous vomiting had ceased, but he was unable to keep either food or medicine on his stomach. The physical signs remained unchanged, except that cœgophony was detected a little above the angle of left scapula. The pulse was 78, small and weak. The pain in the left side was less marked.

On the 22nd things remained substantially unchanged.

On the 23rd it was reported that the patient had passed a restless night, and had vomited several times. The sputa were of a slightly rusty colour. The physical signs in the left chest remained unaltered: but on the right side, posteriorly and near the base, there was a small space over which fine crepitation could be heard. The temperature was still *rather less than* 98° . There was rather sharp diarrhoea.

On the 24th it was again reported that the night had been restless, and the patient had been sick several times. The pain in the left side was now much diminished; and at the posterior aspect of the left lung tubular breathing was less marked: otherwise the state of this lung and pleura was unchanged. It was reported that there had been some tendency to delirium. The general appearance of the patient was a little improved, pulse somewhat stronger—80; temperature still slightly below normal.

25th.—Patient seems much the same in his general state. Has vomited once or twice during the night: sputa still tinged

with blood; tubular breathing over the same small space, on the right posteriorly, where crepitation was heard yesterday and the day before.

27th.—The vomiting came on badly during last night, and could not be checked by medicine. A fresh patch of herpes has appeared below the chin. Temperature still rather below normal; pulse 70. Breathing on right side, behind, nearly natural: no crepitation or tubular breathing. Sounds on left side indicate some advance in resolution of the lung. During the last days delirium has increased, especially towards night.

28th.—Patient is much worse to-day. Pulse very slow; respiration slow and sighing: the general state is profound collapse. Vomiting continuous.

29th.—All the general symptoms worse. The skin is now strikingly cold to the touch, and the breath is markedly cold. Vomiting has ceased.

30th.—Patient died this morning at seven, after a night of rambling delirium, in which he bade good-bye to his friends, accused the house physician of killing him, and broke into laughter and nonsensical talk, alternately. No post-mortem examination could be obtained.

Such is the bare outline of the clinical phenomena. It should be added, however, that the urine contained abundant chlorides on the day of the patient's admission, and that from first to last there was no albuminuria.

As regards treatment, there is nothing to record save unavailing attempts to meet general symptoms. Stimulants (in moderate doses, cautiously increased) entirely failed to strengthen the pulse or improve the general condition, and were vomited, like the food and medicine.

The sickness spontaneously subsided somewhat in the first day or two, but when it returned various palliatives (ice, hydrocyanic acid, counter-irritation, opium) were tried quite vainly. For the last three days the patient was supported by food and brandy frequently injected *per anum*, but no symptom of rallying appeared.

Remarks.—This case must certainly be regarded as very unusual. One is accustomed, with good reason, to regard acute pleuro-pneumonia (the pneumonia and pleurisy being, as here,

almost entirely confined to one side) as far from a fatal affection, when occurring in a young and robust subject who is free from pre-existent organic disease, as this man to all appearances was. The first lessons derivable from the case are a confirmation of Wunderlich's warning as to the dangerous import of sub-normal temperatures in inflammatory affections which involve serous membranes, and a contradiction *pro tanto* of the notion (entertained by Griesinger and Geisler) that pneumonia attended with herpes is of favourable prognosis. When we come to consider the questions which were left doubtful by the unfortunate failure to obtain a post-mortem examination, the first thing which claims our attention is the obstinate and violent vomiting. There can scarcely be a doubt that this was due to very severe catarrh of the stomach,¹ at any rate in the early part of the case: whether at a later stage it was not caused, or aggravated, by *cerebral lesions*, may perhaps be doubted. This severe gastric catarrh must in all probability have largely contributed to the fatal issue, by means of the vomiting it caused; and it is an interesting question, which the absence of dissection prevents our deciding, which of two processes was that by which the vomiting killed. Pure exhaustion of muscular strength may, I think, be put out of the question, considering the vigorous constitution of the patient and the short course that the case took; the probabilities lie between slow coagulation blocking up the pulmonary artery, and slow deposition of fibrinous matter in the vessels of the brain and medulla oblongata.² Either of these events would be directly favoured by the great and prolonged feebleness of circulation which the vomiting produced.

There is another possible theory, that of direct embolism by matters derived from the inflamed lung: and in the early days after the patient's admission I was tempted to believe that this had taken place; but I think that the course of things was, on the whole, too slow to be explained on this hypothesis.

Seeing that, on the whole, the gastro-intestinal catarrh, and not the pleuro-pneumonia itself, was probably the cause of the

¹ The diarrhœa, on the 23d, must be looked on as another symptom of a gastro-intestinal catarrh.

² My friend and colleague, Dr. Radcliffe, was inclined to favour the latter view.

fatal issue in the case of J. P., and seeing that this kind of complication is independently known, from the evidence of Huss and others, to exercise a very unfavourable influence on the course of pneumonia, a question of most practical interest with regard to treatment is opened. It may well be doubted whether the most skilful treatment could have saved my patient at any time after his coming under my care: but I suspect it might be very different in the earlier stages of a pneumonia complicated with severe gastric irritation: indeed there is some experience which points to this conclusion. The subcutaneous injection of morphia, in $\frac{1}{6}$ -grain doses, repeated two or three times in the first twenty-four hours if necessary, appears to exercise quite a peculiar effect in checking violent gastric catarrhs at the outset: and I submit that this is an especially important object to effect where the gastric affection complicates a pneumonia or pleuro-pneumonia.

Reviews.

Niemeyer on the Symptomatic Treatment of Cholera. Translated by P. W. LATHAM, M.D., F.R.C.P., Fellow of Downing College, Cambridge; Physician to Adderbrook's Hospital, &c. &c. 8vo. pp. 57. Cambridge: Deighton and Bell.

IN view of the by no means trifling probability that cholera may visit this country during the approaching autumn, the publication of this essay is a very seasonable act, for which the accomplished translator should be warmly thanked. We realise, perhaps, more fully than before, what manner of man the lamented Felix von Niemeyer really was, now that he is taken from us: his death is felt to be one of the greatest losses that European medicine has sustained during the present century. It has been constantly observed that the high qualities of men of Niemeyer's intellectual rank are as clearly seen in their smallest works as in their most elaborate treatises; and one sees this well in this little pamphlet: there is a sense of power and of clear direct insight impressed upon the reader before he has turned half-a-dozen pages.

It is certainly impressive, not to say startling, to find a consummate clinical observer like Niemeyer laying down, as the plain result of his great experience, the following propositions, which might well stagger some of our bold English theorists. He declares that, notwithstanding conflicting speculations, there is only one clinical symptom of cholera that has the least guiding value—the diarrhœa; only one pathological fact which is at once proved and has an obvious bearing on the course and termination of the malady—viz., the intestinal lesion; and only one sort of treatment—the empiric management of this intestinal catarrh—that can be shown to have produced sensibly good effects. He speaks with the firmest conviction of the arrest of cholera in the inaugural diarrhœa stage by appropriate remedies; and he utterly discredits all the stories of *cholera sicca*, affirming that in these cases the intestines are simply distended with the undischarged fluids.

It is of great interest to learn the system on which Niemeyer, one of the most cautious and sceptical of men, was led to treat cholera. In the early stage of not very severe diarrhoea he gave a few doses of laudanum, and these often checked mild cases: but if the amendment were not rapidly perceptible, he abandoned the opium, and had recourse to calomel (a grain every half-hour) and cold wet packing. He says it was not often that these measures at once arrested the symptoms; yet it sometimes happened so; and he gives one very striking illustrative example. Most commonly the improvement, though decided, was more gradual; the amount of epithelium in the stools diminishing, their colour becoming darker, solid feculent matter appearing, by degrees, in increasing quantity. That these changes were dependent on the treatment, Niemeyer claims to have proved by trying the effects of intermitting the latter; this intermission was always followed by relapse, but improvement often occurred when the treatment was resumed. He says, too, that the benefit of these measures was most strikingly apparent when there was violent vomiting; this was more rapidly checked than the diarrhoea: the alleviation of distress was so just that the patients cried out for the renewal of the cold as soon as the bandages became at all warm, and the effect of the application was very often to send them to sleep.

There are various other interesting details as to the treatment of particular symptoms, but the above are the main points. Niemeyer believes that the essential fact in cholera is a catarrhal inflammation of the intestinal tract: and he treats that inflammation with the remedies which modern experience has shown to be most effective in catarrhal inflammations of viscera generally. He claims that it directly and powerfully relieves symptoms, that it sometimes appears to arrest the disease at once, and more frequently to do so in a few hours or days. He allows that (although his own opinion lies that way) it is impossible to prove that his mode of treatment notably diminished the mortality, since there are no reliable statistics, anywhere, of cholera mortality.

It must be allowed that there is something puzzling and even disheartening in the spectacle of men like Niemeyer upon the one side declaring that intestinal catarrh is the only pathological fact in cholera that we really know, and hosts of other persons on the other side insisting on there being the plainest evidence of general blood-poisoning; some, as we know, proceeding to the most extraordinary deductions as to the mode of action of the poison, and the efforts of nature to eliminate it. But of one thing at least we feel quite certain, that to a calm unprejudiced mind the practical concurrence of such experienced and able physicians as Niemeyer and (in a different way) Macnamara

against the idea of a curative influence of the diarrhœa, ought to be fatal to any belief in that idea. Were the discharges in cholera curative in intention, and actually curative to any considerable degree, how can we believe that the fact could admit of such dispute? What sort of therapeusis by Nature is that which leaves 50 per cent. of untreated cholera patients to die; and even when Nature's efforts and suggestions are seconded by the eagerly obedient eliminationist physician, still leaves the benefit so extremely doubtful, that, to Mr. Macnamara's apprehension (though he was previously prejudiced *in favour* of elimination), the patients seemed to die under the treatment like rotten sheep? On the other question—whether there be, or be not, a real *blood-poisoning* in cholera—we may well admit that there is still the widest scope for doubt: and may add, that to our apprehension, so far, the evidence has always seemed on the whole to point in the affirmative direction. But that the course of the poisoning, if such there be, follows that neatly-arranged programme which has been attributed to it by Dr. George Johnson, it is entirely impossible for us to believe, in view of the utter break-down of the practical proof: and here we may refer to an excellent remark of Niemeyer's on the worthlessness of all that has been built on the supposed early arrest of the biliary secretion. Where, says Niemeyer, is there any proof of the arrest? None has been given; and the mere fact of the disappearance of the usual colour of the stools may well be due to their mere dilution: in fact, there is no reason for supposing that the bile ceases to flow one whit sooner than any other secretion.

It remains for us just to point out the fact that Niemeyer is a decided believer in that vital shedding of the intestinal epithelium in cholera, which is disputed by so high an authority as Dr. Parkes, and, now again more lately, by two of his most distinguished pupils. Our own judgment (especially since we read Beale's papers and the early chapters of Klob's¹ work) has always strongly inclined to the belief that the cholera discharges are largely composed of shed epithelium. But it would surely be a mistake to suppose, as some appear to do, that the proof that the epithelium is not actually shed till after death would make it impossible that the essential lesion in cholera is a catarrhal inflammation of the intestines, and that all the other phenomena are secondary to the latter, and the chances of death proportionate to the extent of the local injury. In truth, however, we are only at the beginning of the discussion between the theories of local and of general poisoning in cholera.

1 "Path. Anat. Studien über das Wesen der Cholera-process." Leipzig, 1867.

Worms: A Series of Lectures on Practical Helminthology.
 Delivered at the Middlesex Hospital by T. SPENCER
 COBBOLD, M.D., F.R.S., &c. &c. 8vo. pp. 178. London:
 Churchill, 1872.

THE humble and unattractive subject of intestinal parasites and their treatment has long found a prophet in Dr. Cobbold, who has shown that sincere and earnest work can elevate even such an uninviting branch of knowledge as this into the regions of intellectual interest. To say the truth, worms are an exceedingly troublesome sort of beasts: and there are not a few persons, even in the "upper ranks" of the profession (save the mark!), who would like to say, if they dared, that the creatures were beneath their notice: the fact being that they have no accurate knowledge about these animals, and feel no confidence in their own power to deal with them. It is no doubt true, as Dr. Cobbold says, that great numbers of medical men still believe, with charming simplicity, that there are but three sorts of intestinal worms, viz. thread-worms, round-worms, and tape-worms; and still prescribe as if they thought that every kind of vermifuge were equally applicable to the expulsion of every kind of worm. In his larger treatises, Dr. Cobbold has elaborately exposed the inaccuracy of such ideas, and has mainly helped to lay, for Englishmen, the basis of a scientific medical helminthology. In his present little work he makes an attempt to simplify and popularise among busy practitioners the more important and practical of his conclusions; and he has thus produced a very useful and handy book of reference on the subject. We recommend it to the notice of our readers, and the more willingly because we do not find that the author disguises the fact of the frequent tediousness of this class of affections, and the numerous relapses which occur, even after the most skilfully devised treatment, in many cases.

Third Annual Report of the State Board of Health of Massachusetts. Boston: January 1872.

THIS very interesting official report contains a number of papers on subjects of great importance. The third special article (after the general report of the Board) is of particular interest and value. It is an analysis, by the Chairman of the Board, the eminent Dr. Bowditch of Boston, of a quantity of correspondence with medical men and others as to the causes of national intemperance, and the most effectual way of providing a remedy for it. This paper is so carefully written, its tone is so moderate, and the author has evidently exercised so much conscientious care in arriving at his conclusions, that it deserves the most

careful attention, not only of medical men, but of legislators and philanthropists. Dr. Bowditch is so well known as a physician of high character, of keen intelligence, and of logical modes of thought, that his conclusions should carry great weight. He is evidently no believer in the reforming power of the total abstinence principle, except for very limited purposes. He examines the sources of intemperance in a philosophical spirit, and he is especially interesting in his remarks on the effects of such influences as climate on the one hand, and the presence or absence, in particular countries, of a plentiful supply of light wines or beers. He plainly disbelieves in the feasibility of any attempts, on the broad scale, to eradicate the popular demand for alcohol; and he brings together much evidence, some of it new and very interesting, in support of the belief that the general introduction of unadulterated light wine and beer would, by supplanting the common use of ardent spirits and of the various and villanous doctored alcoholic compounds which are so extensively consumed, effect an immense improvement in the sobriety and general morality of America and of England. We would particularly call attention to the remarkable evidence that he produces as to the beneficial change which has been worked by such means in the social condition of California.

There are many other papers of much value in this volume, but we have selected Dr. Bowditch's paper as at once the most interesting and important. A cognate subject to that which he treats is also dealt with in a very instructive manner by Dr. F. S. Oliver, viz. the abuse of opium, which appears to be a growing vice in the States. It is an interesting fact that as regards some of the reformatory measures proposed, especially the desirableness of seclusion for persons hopelessly given over to narcotic indulgence, Dr. Bowditch arrives at the same conclusions regarding alcohol as Dr. Oliver expresses with regard to opium.

Clinic of the Month.

Treatment of Small-pox by Vaccination.—Several communications and letters have been published on this subject in the *Lancet*. Mr. Furley observes that, in his opinion, the doctrine universally held that vaccination will be of “no use” if delayed till five days after the inhalation of the germ of variola, is incorrect. For some time past he has been in the habit of vaccinating every case of small-pox that has come under his care, and the results he has obtained are very encouraging, and seem to indicate that vaccination is not only prophylactic, but *curative*. He introduced the vaccine lymph by means of Dr. Wood’s hypodermic syringe, and latterly he has used a hollow charged needle. He finds this treatment more successful in the youthful than in the adult period of life, owing, he thinks, to adults requiring more of the remedy. It is more effectual the earlier the operation is performed. He had five deaths in sixty cases; in three of those a fatal issue was anticipated from the first. Dr. Macintosh gives various cases supporting Mr. Furley’s statements. Dr. Marshall writes, that it is important to remember that the incubative stage of variola is thirteen days, whilst vaccination, as far as his observation goes, is a local disease until some time between the eighth and eleventh days; it is only then that the constitution becomes affected, and it is so from absorption of the lymph in the matured vesicles. He arrives at this conclusion from observing that a person exposed to the infection of small-pox within eight days after being vaccinated may take the disease; but no matter how much exposed after the eleventh day, he will not take it. Also, if a person who has been vaccinated be re-vaccinated within seven or eight days, the re-vaccination will take in some modified way; but if done after the eleventh day, not at all. Hence, if an unvaccinated person, who has been exposed to the infection of small-pox, be vaccinated in time for the vesicles to rise and the lymph to be absorbed before the symptoms of small-pox begin, he will either have no attack, or, what he thinks is more common, will have severe symptoms, ending with the appearance of a few spots, sometimes of none. But if the symptoms of small-pox commence before the vesicles have risen and begun to desiccate, he will have a severe attack,

but modified in so far that there will be no secondary fever. (*Lancet*, May 25 to June 15.)

Spina Bifida cured by Injection.—Dr. Morton, of Glasgow, records two cases in which spina bifida was cured by injection. The procedure may be shortly stated to consist (1) in performing two tentative tappings with a grooved needle, with an interval of four or five days between the two. (2) The tumour is tapped with a small trocar and canula, allowing about half the contents of the sac to escape, and about a drachm of a solution of iodine in glycerine was injected. The after-treatment consists in dressing with some bland substance, as oil or lard, cleanliness, and care, so as to avoid local injury or irritation. Prevention of the complete escape of the cerebro-spinal fluid he believes to be of the greatest importance; and in the second case he brushed the wound over with collodion, which answered admirably. The presence of this fluid is essential to the functional, if not the structural, integrity of the spinal cord and brain; and if it be allowed to drain away, the child speedily succumbs. (*British Medical Journal*, June 15, 1872.)

Deductions to be drawn from the Extinction of Typhoid Fever in Millbank Prison by the Disuse of Thames Water.—Surgeon-Major A. C. C. de Renzy, after entering into considerable detail in regard to the mortality formerly occurring in Millbank Prison, and the circumstances that have led to its extinction, considers the facts he has gathered together warrant the following conclusions:—1. That the extinction of typhoid fever, and other diseases of the same class, is quite within the range of practicability. 2. That the extinction of one class of zymotic diseases is not necessarily followed by zymotic diseases of a different class. For example, it is supposed that the increased prevalence of scarlatina and measles of late years is due to the partial displacement of small-pox by vaccination. The case of Millbank shows that it is practicable to protect a community against every kind of zymotic disease. 3. That as some of the ablest physicians in London failed for many years to detect the true cause of the unhealthiness of Millbank Prison, and assigned causes for it which later reference has found to be unconnected with it, it is probable that other zymotic diseases might be entirely removed by the general disuse of impure water. 4. That as it required many years of observation to establish this noxious influence of Thames water at Millbank, even when well filtered, under conditions very favourable for detection, we should be cautious in accepting the opinion, based on the results of chemical analysis, that the use of that water by the population of London is free from danger. 5. That the vital statistics of prisons, carefully kept and tabulated, would prove of great value

as data for the investigation of the causes of disease. (*Lancet*, June 15, 1872.)

Treatment of Fissure of the Anus.—A good lecture on this subject by Dr. Dolbeau, reported by Dr. Osborne Powell, is contained in the *Medical Times and Gazette*. He observes that each evacuation of the bowels is accompanied by a feeling of something cracking, followed by intense pain; nevertheless, in many instances no fissure can be found, even after the most careful and minute inspection. He consequently regards it as of the nature of neuralgia. It commonly occurs between the age of eighteen and thirty, and most frequently in women. In the treatment the first thing to do is to abolish the muscular element. Récamier, whose name is inseparable from the history of this malady, well comprehended that the fissural lesion is an element without importance, and he cured several patients by what he termed *cadenced massage*. Chloroform was then unknown. He had the patient held by some strong assistants, and he introduced first one finger, then another, and so on until he had passed his whole hand into the rectum; he then closed his fist, and drew his hand from the intestine. This was a most rational operation, and the employment of chloroform in the present day permits us to practise it in a much less barbarous manner. Thus, the patient being put under the influence of chloroform, the surgeon introduces his two thumbs into the rectum, and endeavours to bring them in contact with the two ischias; at that moment one hears a strong cracking noise, and the operation is finished. The first stool after the operation is painful, but this pain is simply the result of the region being contused, and does not in the least resemble the pain which characterised the spasmodic attacks. The succeeding stools are without any pain, and the cure is assured. (*Medical Times and Gazette*, June 8, 1872.)

Treatment of Asthma.—The treatment of asthma, as Dr. Williams observes, must vary much in its simplicity and success according to the unity or complication of the disease. Against the bronchial spasm we have remedies which are pretty effectual in most cases. Belladonna and stramonium rarely fail to relieve the bronchial spasm; and in transient cases, where this is the only element, they may suffice to cure the disease. The extracts are the most reliable preparations, and may be given in doses of from a quarter of a grain to half a grain every three, four, or six hours, whilst the tendency to spasm lasts. The dryness of the throat, which both these drugs often cause, may be counteracted by frequently sipping linseed-tea or barley-water. Sometimes, however, this dryness is useful in moderating the catarrhal flux which may follow the spasm. But in most cases there exists

something more than the mere spasm; and therefore we commonly have to give these antispasmodics in combination with other remedies. Thus, often there is an inflammatory cold, calling for the addition of salines and counter-irritation; and this may amount to bronchitis, requiring the aid of small doses of tartarised antimony. In chronic cases, when the attacks have recurred frequently or lasted long, there is no combination more beneficial than that of iodide of potassium, in two or three grain doses, and ten or fifteen grains of bicarbonate of potass, with the stramonium or belladonna. Dr. Williams believes that he speaks within bounds when he says that with a combination of this kind he has cured or greatly relieved hundreds of cases of asthma. The diuretic or eliminative action of these medicines may be advantageously increased in some cases by the addition of squill, colchicum, or tincture of cantharides, particularly where there are indications of gout or of diseases of the skin. On a similar principle, in chronic cases certain mineral waters are sometimes useful, particularly those of Eauxbonnes and Cauterets in the Pyrenées, Vichy, and Ems. There are several other remedies for asthma in common use, generally much inferior in efficacy to the preceding, but occasionally useful as subsidiary aids, and sometimes they are our chief resources where those disagree. Such is the ethereal tincture of lobelia, which in doses of from twenty to sixty drops he has known in a few instances to be quite successful; but more frequently it has failed, and sometimes caused much nausea and discomfort. Indian hemp, in doses of a grain of the extract, gave signal relief in two cases; but in others it quite failed, and sometimes caused distressing disturbance of the brain and head. Smoking cigarettes of stramonium, or of the *Datura tatula*, inhaling chloroform (which for safety should be mixed with sulphuric ether and alcohol), and breathing the fumes of burning nitre-paper, are expedients which often give relief in individual cases; and although this relief is less complete and permanent than that following the use of the remedies first recommended, yet they may be useful when these fail, and, being prompt in operation, may be employed to ward off slight attacks, where stronger agents are not required, or before the latter can be brought into effective operation. In some cases change of air succeeds wonderfully, and this not always when the change has been of the most salubrious character. In fact, the caprices of asthma with regard to air are very curious, and can hardly be accounted for. In most instances, however, a dry atmosphere agrees better than a damp one, and the air of a large town better than that of the country, especially if this be low and damp. Of places in which he has known asthmatics to be most free from attacks, he mentions London (several places of the West-end), Tunbridge

Wells, Clifton, Brighton, and Margate ; in summer abroad—Paris, Pau, and Hyères. (*Medical Times and Gazette*, June 8, 1872.)

Treatment of Hernia by Aspiration.—At the meeting of the Académie de Médecine, on May 21, M. Demarquay presented a man, twenty-one years of age, in whom he had reduced a strangulated congenital inguinal hernia by aid of aspiration. On May 5 a tumour appeared in the left groin, accompanied by severe pains and vomiting, which persisted next day. At the end of twenty-four hours he was taken to the Paris Maison de Santé, where the taxis was employed without success. Ice was applied during the next twelve hours, when M. Demarquay saw the patient. His features had undergone great change, and fever was set up. A congenital, elongated, voluminous inguinal hernia was found to exist, and M. Demarquay paid the more attention to other measures, inasmuch as he had never succeeded in curing this description of hernia by operation. He applied carefully the taxis, while the patient was put into a deep sleep, with no effect, and he determined to try the effect of removing the intestinal liquids and gases by means of aspiration. A fine trocar was passed into the centre of the tumour, and by means of Potain's aspirator about 120 grammes of intestinal liquid were drawn into the recipient. The tumour subsided completely, and the trocar having been removed, some minutes were allowed to elapse without touching the tumour, in order to observe whether new liquids or gases would enter the strangulated intestine. No renewal of the tumefaction took place, and very slight pressure upwards sufficed to procure the return of the intestine into the cavity of the abdomen. The patient was kept quiet and on low diet, fractional doses of opium being administered. No ill consequence followed. The case M. Demarquay regards as striking, and he proposes to apply this new mode of treatment:—(1) To all congenital hernias, and to recent hernias which become strangulated at the time of their formation. (2) To old hernias which were quite reducible a few days prior to strangulation, and in large umbilical hernias that have been recently strangulated. (3) Aspiration, which has for its object the facilitating of the employment of the taxis, should only be employed at an early period, when one can be well-nigh certain of returning into the abdomen the intestine in an unaltered state, and capable of resuming its functions. (*Ibid.* June 1, 1872.)

Extracts from British and Foreign Journals.

Diagnosis and Treatment of Cerebral Disease originating in Syphilis.—In a paper having a similar bearing to the extract just given, Dr. Owen Rees observes that cases of cerebral disease originating in syphilis are very generally diagnosed, notwithstanding the difficulty occasionally met with as the result of reticence on the part of the patient. We are indeed often assisted by observing eruptions, nodes, cicatrices, and ulcerative destruction of parts, or suspicious rheumatic pains may be complained of; but these indications are not always afforded to us, and the history which would help to a correct diagnosis is not unfrequently withheld. Even when thus unassisted, points may be recognised in the case which become sure indications of the nature of the disease, amongst which are the following:—1. The paralytic seizure is generally the immediate result of some violent exertion, or of some long-continued muscular effort carried on to fatigue, and the collapse is often so great as to threaten immediate dissolution. 2. The symptoms may be hemiplegic or paraplegic, and in the latter case we frequently have no reason to suspect the spinal cord, though in other cases that structure may be seriously involved. 3. The hemiplegic and the paraplegic symptoms are peculiar in character, there being marked irregularity in loss of motor power and of sensation as affecting opposite sides of the body. Thus, the right leg and the right arm may be paralysed, the leg being scarcely affected, while at the same time the left leg may have lost motor power. Again, there may be hemiplegia of one side and anæsthesia of the other, and the loss of sensation may perhaps only affect one limb. Such irregularities occur in great variety. 4. Pain in the head and tenderness of the scalp are scarcely ever wanting. It is worthy of note that anæsthesia is very generally present in syphilitic cerebral disease, often remaining in slight degree long after every other symptom has passed away. The higher vascularity of the grey cerebral matter, rendering it more likely to become infected by the deposit of diseased blood, may perhaps explain the condition. 5. Aphonia has been observed in many cases in the early stage, greatly increasing the difficulties of the diagnosis. In regard to treatment, Dr. Rees has been strongly impressed with the belief that

mercurial treatment is essential to the removal of the gummous deposits of syphilis, and that the patient may be in great peril even when the cause of evil is recognised, if the practitioner be satisfied with relieving the symptoms which present themselves, without having regard to the probable existence within the cranium of material still unabsorbed and ready to produce violent symptoms whenever an exciting cause may come into action. Syphilitic deposits in the brain may, just like various forms of malignant disease, be present in certain parts of the brain matter without producing marked symptoms. The intervention of an exciting cause induces inflammation, the deposits occurring as the results of this action being composed of the ordinary components of the blood. These materials are easily absorbed by the iodide of potassium; the symptoms disappear, and the patient is considered to be perfectly cured. The original syphilitic deposit, however, still remains unabsorbed, and a recurrence of symptoms is imminent unless recourse be had to *mercurial treatment* in order to effect the removal of the gummous mass. The power of the iodide of potassium to effect the absorption of homologous growths, such as those observed on bone, is beyond all doubt, and probably we may confidently trust to its use in all cases of new growth of connective tissue, where the mere proliferation of cells has not been succeeded by fibrillation and an approach to retrograde metamorphosis. These changes occurring, the formation of gummata seems to place such tumours beyond the power of the iodide of potassium, whilst mercury appears to be their special antagonist. (*Guy's Hospital Reports*, Ser. III. vol. xvii.)

Period of Incubation of Typhus, Relapsing Fever, and Enteric Fever.—Dr. Murchison remarks that the period of incubation of contagious diseases, or the period that elapses between the entrance of the contagium into the system and the first manifestation of its presence there by symptoms, is not merely a matter of scientific interest, but has many practical bearings, and lies at the bottom of many of the most important sanitary questions. Cases throwing light on the latent period of a disease are of a threefold nature: 1. Those in which there has been a single limited exposure to the contagium. 2. Those in which there has been a protracted exposure both limits of which are known. 3. Those in which there has been a protracted exposure of which only one limit is known. Either the exposure has persisted from the commencement up to the date of the patient's seizure, in which case it can only be said that the period of incubation has not been less than that interval, or an interval of time has elapsed between the cessation of a protracted exposure and the commencement of symptoms, and

then it can only be said that the period of incubation has not been less than that interval. The first class of cases are of course the best. Dr. Murchison gives the data on which his conclusions are founded, and states that he has been engaged for many years in collecting them. In regard to *Typhus Fever* he believes he has ascertained—1. That the period of its incubation varies in duration in different cases. 2. In a large proportion of cases it is about *twelve days*. 3. In exceptional cases it is longer than twelve days, but it rarely if ever exceeds three weeks. 4. In many cases (one-third or more) it is less than twelve days, and occasionally there is scarcely any latent period, the symptoms commencing almost at the instant of exposure to the poison. In regard to *Relapsing Fever* the facts obtained are somewhat less satisfactory, but they point to the following conclusions:—1. That the period of incubation of relapsing fever is not a fixed period, and is even more variable than that of typhus. 2. It is, on the whole, shorter than that of typhus. In not one of the nine cases in which it was accurately determined did it exceed *nine days*, and in a considerable number of cases it did not exceed five days. 3. Occasionally, as in typhus, there is scarcely any latent period at all, the symptoms commencing almost immediately after the first exposure to the poison. In regard to *Typhoid* or *Enteric Fever*, Dr. Murchison states that reliable facts bearing on the period of incubation of enteric fever are even more difficult to obtain than illustrations of the latent period of typhus or relapsing fever; his conclusions are—1. That the period of incubation of enteric fever is most commonly about *two weeks*. 2. Instances of a longer duration appear to be more common than in typhus or relapsing fever, though in cases where it has been reported to extend over many weeks or months it may be doubted whether the disease has not had an independent origin. 3. The period of incubation is often less than two weeks, and, as in typhus and relapsing fever, it may not exceed one or two days. (*St. Thomas' Hospital Reports*, 1871.)

Treatment of Diabetes.—Dr. Richardson, who was himself a sufferer from this disease, effected a cure in himself, and subsequently in various patients under his care, by adopting a plan of which the following are the essential features:—The employment of regular and steady exercise; ablution of the skin daily with soap and water; the use of a bath containing a table-spoonful of carbonate of soda twice in a week; exposure of the surface of the body as far as is practicable to the sunlight, and the continuous use of iron, which he uses in the form of tincture of the perchloride in four or five drop doses, with one or two drops of tincture of *nux vomica* and eight or ten grains of chlorate of

potash three times daily. He is an advocate of a restricted diet; but when the plan of treatment which he suggests is carried out fully, he finds that a considerable amount of relaxation as regards food is not injurious. He regards the sudden adoption of a very restricted diet as likely to prove highly prejudicial. Dr. Richardson's present dietary is sufficiently liberal, and, besides meat, includes brown bread with plenty of fresh butter, macaroni, and rice, potatoes sparingly, and at times a little dry fruit. Even a few glasses of champagne occasionally he does not find at all injurious. (*Dublin Journal of Medical Science*, April 1872.)

Sand Baths.—The sand bath is applied either by allowing the sand to fall upon the body or by rubbing it into the skin, or by imbedding the body in a mass of it. The sand is usually warmed either artificially or by exposure to the sun, and is either dry or moist. The amount of sand used for an adult in the sand baths at Dresden is from 10 to 12 Eimer, and for a child from 4 to 6.

Dry sand contains more heat than an equal volume of water; it loses no heat by evaporation. The interchange of heat between sand and the body does not take place with the same activity as between water and the body. According to Flemming, when the sand is raised to a temperature of 40 to 41 C. (104° to 106° Fahr.), it is described in the first instance as cool, and after a time as warm, the communication of heat obviously taking place but slowly. The bath is called warm when it is applied at a temperature of 117° Fahr. or rather less; in the course of a day or two it can be borne 3° or 4° higher, and ultimately 122° Fahr. comes to be regarded as normal and pleasant. There are, however, many exceptions to this rule. Irritable persons with sensitive skins can only bear a lower temperature; others again with cooler, thicker, and more œdematous skins can support a temperature of nearly 130° Fahr., at least when applied to a part only of the body. Sturm's experience resembles that of Flemming. According to Cordes, when baths of from 90° to 120° Fahr. are felt to be warm to the body generally, still hotter sand may be applied to painful limbs or to parts locally affected. A peculiarity of the sand bath is, that different temperatures can be applied with ease to different parts of the body at the same time. The sand is thus applied in the Dresden Institute:—A layer five inches in thickness is placed at the bottom of the zinc pan or basin, and the patient is directed to lie upon it. If he desires to have a half-bath only, the upper part of the body is covered with a woollen jacket. The attendant then pours over the extremities and pelvic region, avoiding making a dust, a layer of sand to the height of five or six inches. Where the whole bath is taken, the

abdomen and thorax are also covered, the layer of sand upon these parts not exceeding a few lines in depth. Each part covered with sand must remain absolutely quiescent, or the sand will run off it. Sturm applies cold-water dressing to the head whilst the body is immersed in the sand, and subsequently directs an ordinary hot bath of 100° Fahr. to be taken. Fleming orders a shower bath at a temperature of 79° Fahr.; Cordes a shower bath or douche of soft or sea water of lower temperature. The duration of the bath varies according to circumstances, from 25 to 50 minutes; half-baths may be prolonged to 60 minutes; most frequently, however, they are not employed for a longer time than three-quarters of an hour. According to circumstances, again, the patient is bathed daily, or every other day, or for two days together, and then an intermission of one day. The duration of the whole bathing period or season varies from one to two months. The physiological effects are, that after immersion or embedding in the sand for a period varying from five minutes to a quarter of an hour, the patient experiences a pleasant sensation of warmth. After the lapse of five minutes more sensible perspiration appears on the uncovered parts of the body, which continues to increase to the end of the bath. Previously to the appearance of the sweat, the skin of these parts becomes somewhat reddened. On lightly dusting away the sand from the parts that have been covered, a layer several lines in thickness remains adherent. These parts do not feel much warmer, but turgescient. Thirst is experienced as a consequence of the sweating. At the end of a sand bath of ordinary duration and temperature, repeated measurements have shown that there is an increase in the temperature of the axilla, amounting in many instances to only a few tenths of a degree, but in others to upwards of 2° or even 3° Fahr. The number of the pulse increases from five to eight beats in the minute. After a bath has lasted from 25 to 30 minutes, arterial pulsation is felt by many patients in different parts of the body.

The therapeutic employment of the sand bath is especially recommended in cases where the activity of the skin is suppressed, as in Bright's disease of the kidneys, in articular rheumatism, sciatica, in chronic gout, scrofula, and in rheumatic paralysis. It is remarkable and interesting that the Arabs take from twenty to thirty sand baths in cases of syphilis, with sparing diet of bread and honey. The patient is buried to his neck in sand, in which he remains exposed to the sun for the whole day, and is allowed towards the evening to take a little food. This burying is prolonged sometimes for a month. (P. M. Lersch, *Polymorphe Balneologie*, Erlangen, 1871; and *Medicinisch-chirurgische Rundschau*, März 1872.)

Congenital Hypertrophy of Left Hand and Arm.—A remarkable case of this affection is recorded by Mr. P. H. MacGillivray, of Bendigo. The disproportion between the two arms was but slight at birth, but at the age of twelve months the left arm began to grow rapidly, the whole limb from the shoulder became much enlarged, to some extent in point of length, but chiefly in thickness. The first, second, and third fingers were enormously hypertrophied, the hypertrophy involving all the structures. The child could use the arm and all the fingers, but could not lay hold of anything. As the most likely way of arresting the growth, Mr. MacGillivray ligatured the brachial artery high up. This was accomplished successfully, and a month afterwards no increase had occurred. The hypertrophied fingers were then amputated with good result, the arm and remaining fingers ceasing to grow, and the child obtaining good use of the latter. Photographs of the limb accompany the paper. (*Australian Medical Journal*, January 1872.)

Treatment of Strictures of the Urethra.—Dr. Otis, of New York, commences his essay by affirming that the slightest abnormal encroachment upon the calibre of the urethral canal at any point in its course is sufficient to perpetuate a urethral discharge, or even under favouring circumstances to establish *de novo* without venereal contact. He refers to and describes the various instruments invented or improved by Mr. Holt, Sir Henry Thompson, Maisonneuve, and others. Their defect, however, he considers to be that they are all deficient in adaptability to the dimensions of the structure upon which operation is required. The operation is performed upon the flaccid urethra; the amount of the resiliency of the stricture is undetermined; the divulsing shaft is selected without exact data, and the appropriate size of blade in the cutting instruments is a matter of judgment and very liable to error. To overcome these difficulties, Dr. Otis has constructed a new instrument which he terms the *dilating urethrotome*, which consists of a pair of steel shafts connected by short pivotal bars, on the plan of the ordinary parallel ruler. The expansion or contraction of these is effected by means of a screw which traverses the handle and is moved by means of the finger button. Attached to the distal end of the screw is a pair of short curved registering arms, by which the degree of separation of the shafts can be accurately ascertained. Up to this point the instrument is simply a *divulsor*, but the upper bar of the instrument, which is hollowed out, is traversed by a urethrotome, which can be fixed at any given point, and then made to protrude by a mechanical arrangement. The especial advantages claimed for the instrument are, that it makes the urethra *tense*, thereby establishing

the stricture as a fixed point; that it is capable of being adapted to strictures of any size within its compass; that it accurately defines their locality and extent; that it attacks a tense instead of a flaccid stricture, and hence that its work is approached with confidence; that its incisions are made with ease at a predetermined point, depth, and extent, instantaneously, and hence with the slightest possible discomfort to the patient; and lastly, that it combines great strength with ease and simplicity of manipulation. Dr. Otis gives drawings of his own instrument, and an appreciative review of those of Holt, Sir H. Thompson, Maisonneuve, and others. (*New York Medical Journal*, February 1872.)

Pain and Disturbances of Cutaneous Sensibility.—A paper with this heading appears in the 54th volume of Virchow's *Archiv*, by Dr. H. Nothnagel, and has been translated by M. Kloman for the *Philadelphia Medical Times*, one of the best of the numerous American medical journals. Dr. Nothnagel observes that with whatever facility neuralgia can be diagnosed in typical cases, as in *tie-douloureux* or *malum eoturnii* (*sciatica*), in others it is difficult to say whether we have before us a rheumatism, an inflammatory affection, or a so-called neuralgia. The main difficulty in deciding this question is evidently whether in recognising this disease, neuralgia, we are obliged to rely upon purely objective symptoms; for unquestionably diagnosis becomes so much the more certain the greater the amount of objective signs accessible to perception by the senses upon which it can be based. In this point of view Türk's observation was of importance, to the effect that in neuralgia there at times exists a hyperæsthesia, but still more frequently an anæsthesia, of the cutaneous surface corresponding to the location of the deeper-seated pains. The only author who has shown some appreciation of this statement is Traube, who says that in all the cases investigated by him the hyperæsthesia or analgesia could be demonstrated, even in the intervals of freedom from pain, indifferently whether the neuralgia was of central or peripheral origin. Dr. Nothnagel states that he has investigated all the cases that have come before him, to the number of sixty or seventy, for some years past, in reference to cutaneous sensibility. He has not included amongst these any cases of painful *internal* affections, but has limited himself to the observation of the painful affections of the superficial nerves, including those of the fifth occipital, supra-clavicular, intercostal, and cutaneous nerves of the abdomen, with the individual branches of brachial, lumbar, and sacral flexuses. He has excluded those forms as far as possible in which a central affection lay at the foundation of the neuralgia, also cases of epilepsy and

hysteria, and cases in which there was a demonstrable anatomical lesion; whilst, on the contrary, he has paid particular attention to those cases which were traceable to catching cold, the questionable cases of neurites with neuralgic phenomena, cases of so-called irradiated neuralgia (for example, in the intercostal nerves in *ulcus ventriculi*), and finally the neuralgias of malarial poisoning.

Now, in all these M. Nothnagel states that he has been able constantly and without exception to corroborate the remarks of Traube, and establish an alteration of the cutaneous sensibility, either a hyperæsthesia—or, more correctly, a hyperalgesia—or an anæsthesia; and it proved to be a rule that at the commencement of the neuralgia there was cutaneous hyperalgesia, but upon longer continuance diminution of sensibility. The various qualities of the sense of touch were tested in the investigation, as its sensitiveness to the prick of a needle, to variations of temperature and pressure, and to the electric current.

Impressions of touch are perceived much more vividly on the affected side than upon the healthy one, so that slight needle-pricks cause pain, and contact with a metallic substance of the temperature of the room may call forth general chills. Yet there is no actual hyperæsthesia, *i.e.* no increased power to distinguish slight *differences* in sensation; it is therefore properly termed hyperalgesia. The anæsthesia that usually occurs in the later periods is comparatively slight, and may easily be overlooked unless the testing is carefully performed. *Partial* paralysis of sensation does not occur in this form of anæsthesia. The disturbance of cutaneous sensibility stands in a relation varying as to its seat with the extent of the spontaneous pains. We often see that it accurately corresponds to the cutaneous distribution of the nerve-branch or trunk to which the pain is referred. Just as often, however, the exceedingly remarkable fact exists, that with a neuralgia of a nerve-branch or nerve-trunk quite limited in extent, the cutaneous hyperæsthesia or anæsthesia appears to extend over the whole of the corresponding half of the body—a fact to which Türk some time ago called attention, and which Dr. Nothnagel states he can fully confirm. From an examination of his cases, Dr. Nothnagel draws the conclusion that in the first two to eight weeks an increased sensibility of the skin exists; then in the after-course the anæsthesia is developed; and further, that not only, as Türk assumed, does cutaneous hyperæsthesia or anæsthesia occur in neuralgia, but that the same may also occur with any deep-seated pain from other causes. Dr. Nothnagel is of opinion that the pain, as such—the physiological process in the perception of pain—causes the changes of cutaneous sensibility, the hyperæsthesia as well as the anæsthesia, and considers it to be probable that we must

seek in the central ganglion cells the changes which cause the anæsthesia and hyperæsthesia of the skin in neuralgia and other pains. (Virchow's *Archiv*, vol. liv., and *Philadelphia Medical Times*, March 15, 1872.)

On the Combined Action of Morphia and Chloroform.—The following note by MM. Labbé and Guyon was lately presented to the Académie des Sciences by M. Claude Bernard:—Although chloroform has now been long in use, it is scarcely ever employed without some degree of apprehension on the part of the practitioner, for no rule or method of proceeding has hitherto been suggested by which we can be placed in a position of perfect security against the accidents that it may produce. To obtain complete anæsthesia whilst avoiding the risks of the fatal issue which has not unfrequently accompanied the inhalation of chloroform, is a problem well worthy of investigation and solution. M. Claude Bernard, in his course of lectures delivered at the College of France, raised and partly solved this question when treating of the physiological action of the different alkaloïds of opium, by showing that a combination of morphia with chloroform produces a very perfect condition of anæsthesia, the quantity of the latter drug being very much smaller than that usually administered. Thus he injected about a sixth of a grain of morphia into a dog, subcutaneously, and soon after gave it chloroform. Anæsthesia was found to be quickly produced and much prolonged. Whilst M. Bernard was making these experiments on dogs, Nussbaum of Munich observed the same results to occur in a woman who had taken a large quantity of chloroform. Not wishing to continue the inhalation of this, he bethought himself of giving her an injection of morphia, and noticed that the insensibility produced by the chloroform was much prolonged. Two surgeons of Strasburg, MM. Rigault and Sarazin, have made researches on this point, and appear to have arrived at the same results. MM. Labbé and Guyon give the details of four operations in which the combination of the subcutaneous injection of morphia and the inhalation of chloroform was adopted. Of these we select the following:—On the 27th January, 1872, M. Labbé performed an amputation of the foot below the malleolus. Twenty minutes before the operation two centigrammes (about $\frac{1}{3}$ grain) of hydrochlorate of morphia were injected beneath the skin; chloroform was then given, and the patient exhibited slight excitement. At the expiration of seven minutes the anæsthesia was complete, and lasted long after the operation, which occupied seventeen minutes, was finished. The quantity of chloroform used was about an ounce. This patient, before sensibility returned, was wide awake, and replied intelligently to questions asked. MM. Labbé and

Guyon think that rather a larger dose of the morphia might be injected, and at rather a longer period prior to the operation. (*Bulletin Général de Thérapeutique*, 15 Mars, 1872.)

Subcutaneous Injection of Ergotin in Varix.—Having observed the records of good results following the subcutaneous injection of ergotin in cases of aneurism in the hands of Von Langenbeck, Schneider, and Dusoit, Dr. Paul Vogt, of Greifswald, was led to try the remedy in varix of the lower limb. The first patient on whom the experiment was made was a man aged sixty, who had suffered for several years from extensive varices of the leg. A solution of two grammes of ergotin was made in seventy-five grammes each of spirit of wine and glycerine, and a quantity containing twelve centigrammes of ergotin was injected at the proximal end of a varix more than two inches long, and as thick as a little finger, lying over the tibia. The injection was repeated every second day. In eight days the varix could not be seen, and in six weeks no trace was left. During the treatment the patient went about as usual. Another varix of the size of a hazel-nut, lying on the outer side of the calf, was treated in a similar manner with the same result. At the point where the injection was made there was some hard circumscribed infiltration, which gradually disappeared. Several other patients in the Greifswald Hospital, some of them with very large varices, have been treated with the subcutaneous injection of ergotin with a surprisingly good result. Dr. Vogt believes that the ergotin causes contraction of the muscular coat of the arteries, so that the flow of blood into the dilated vessels is hindered; that it also produces contraction in the veins, and that the local infiltration following the injection may have some effect by the compression which it exercises. (*Berliner Klin. Wochenschr.*, March 4, 1872, and *Brit. Med. Journal*, April 27, 1872.)

Injection of Tincture of Iodine and of Alcohol in Bronchocele.—Dr. Karl Schwalbe (*Virchow's Archiv*, Band 54) speaks of the injection of tincture of iodine and of alcohol in the treatment of thyroid tumours. Those in which it succeeds best are the soft and cystic enlargements; in hard fibrous tumours no result is obtained; and in vascular swellings there is danger of the production of embolism, unless the vessels have been partially contracted and the tumour diminished by the internal use of iodine and by electrolysis. If blood escape in a stream on the introduction of a canula, the injection must not be made; in any other case it may be done with safety. About ten or twenty drops are injected as nearly as possible in the middle line of the neck, over the tumour, by means of a syringe fitted with a canula having a short point. If no blood flow on the intro-

duction of the canula, the fluid is forced in, and before removing the instrument the operator waits for a short time, in order that the injection may not again flow out. The operation is sometimes followed by transient pain in the course of the nervus auricularis magnus, cough, gastric oppression, vomiting, and, in weak subjects, fainting. One woman who was subject to epilepsy was seized with symptoms of hemiplegia soon after the injection, and died in convulsions. Schwalbe prefers alcohol for the injection, a drachm of iodide of potassium being at the same time taken daily. The injection may be repeated at weekly intervals. No confinement is required during the treatment, the average duration of which is two or three months. (*Wiener Medizin. Wochenschr.*, March 16, 1872, and *Brit. Med. Journal*, April 27th, 1872.)

Treatment in Phosphorus Poisoning.—An interesting paper on this subject is contained in the *Bulletin Général de Thérapeutique* by Dr. Rommelaere, where it is stated to be a portion of an excellent memoir by the author, on the general subject of poisoning by phosphorus. The different modes of treatment may, he says, be arranged under three heads:—1. To prevent the absorption of the phosphorus: (*a*) by its expulsion by emetics; (*b*) by diet, abstaining from eating any fatty substance; (*c*) by investing it with a layer of copper (Bamberger's method). 2. To prevent the effects of phosphorus on the blood: (*a*) by means of ether (Bellini's method); (*b*) by the administration of alkalies; (*c*) by oxidising agents; (*d*) by transfusion of blood. 3. Treatment by oil of turpentine.

In reference to these several plans of treatment, it may be said that if the case is seen early, and in those cases where the poison is known to have been swallowed in the form of heads of matches, emetics may be employed; but it must be remembered that if some time has elapsed, phosphorus produces violent inflammation of the membrane of the stomach, a condition not likely to be improved by the administration of emetics. In regard to abstention from fat, it is a point of capital importance in the treatment of cases of poisoning with that substance, and the ingestion of a quantity of oily matter may perhaps account for some cases of relapse when the case appeared to be complete. Sulphuric ether in the form of vapour appears to be useless, and no better results have been obtained from the use of alkalies. Oxidising agents like permanganate and chlorate of potash are positively harmful. In regard to transfusion of blood, Eulenberg and Landois have proposed and practised this plan in animals, and have then found it tolerably successful. The treatment of phosphorus poisoning by means of oil of turpentine was first brought prominently into notice by Dr.

Letheby, who found that in the lucifer match factory of Black and Bell, at Stafford, the workmen were protected from the action of the phosphorus by wearing a little open phial round the neck, partly filled with oil of turpentine. Oil of turpentine vapour, it is well known, inhibits the combustion and phosphorescence of phosphorus. Bellini made various experiments with oil of turpentine, which led him to report unfavourably of its use in poisoning by phosphorus. A remarkable case that occurred under the care of Dr. Andant, of Dax, recalled attention to it. A man desirous of committing suicide chewed about 120 match-heads for some minutes, and, to make assurance doubly sure, swallowed about half an ounce of oil of turpentine, but to his surprise he experienced no other symptoms than a severe belly-ache. MM. Andant, Köhler, and Lichtenstein have reported other similar cases in which oil of turpentine was equally efficacious. It hence appears to be a very valuable remedy, but the precise mode in which it acts has not been clearly ascertained. (*Bulletin Général de Thérapeutique*, liv. iv. 1872.)

Treatment of Poisoning by Camphor.—Dr. Joseph Beck, of Indiana, reports two cases of poisoning by camphor. This substance, he says, was formerly regarded as *either* a stimulant or a sedative, but now most pharmacologists regard it as being primarily stimulant, its action being chiefly directed to the nervous system; and as secondarily sedative. In one of his cases he found a well-grown child, three years of age, lying on his mother's lap in strong convulsions. The scalp was intensely hot; extremities cold; the abdomen retracted, and the patient apparently moribund. There was nothing at first sight to indicate the cause of the convulsion, and the mother could give no explanation of it; but on approximating the child's mouth Dr. Beck perceived a strong odour of camphor. It was soon ascertained that the child had begged for a fragment of some of this drug which the mother had been putting away seven or eight hours previously; and Dr. Beck at once concluded that he had to deal with a convulsion and cerebral disturbance due to the secondary action of the drug, namely, powerful sedation. As the child was entirely unable to swallow, it was ordered an enema of two ounces of whiskey mixed with two ounces of thin starch, to be repeated in two hours if necessary; also bags of pounded ice to the scalp until the convulsion ceased. Two hours and a half after the child had entirely recovered, and the second enema was not required. The second case occurred in an adult, and was similarly treated with success. (*St. Louis Medical and Surgical Journal*, May 1872.)

Practical Observations on the Use of Carbolic Acid in Syphilitic Diseases.—Dr. Karl Sigmund remarks that, incited

by the reports of the value of carbolic acid in parasitic affections, he determined to give it an extensive trial in syphilitic affections. His results, however, appear to have been essentially negative. He says, its internal administration produces no effect whatever upon the progress or cure of syphilitic diseases. Large doses—as 10, 15, or 20 grs.—could not be administered for several days together without much disturbance of the digestion and of the circulatory and urinary systems, which is of so much the greater importance in syphilitic cases, because many of the patients are very badly nourished, and the use of known antisiphilitic remedies has often to be postponed for weeks, till the general health is to some extent improved. The fungoid theory of the origin of syphilis is certainly in no way proved; but Sigmund's experiments have convinced him that, even if it were, the use of carbolic acid for six or eight weeks after the existence of primary syphilis has been clearly diagnosed, has no influence upon its development. Secondly, the acid has been used as an external application on account of its disinfecting properties, either pure or in solution in spirit or in glycerine. When applied pure, it changes the tissues into a dense scab, which separates more slowly than after the use of other escharotics, and in this and other respects presents no advantages over other corrosives, as sulphate of copper, red precipitate, chloride of zinc, and Vienna paste. When applied weak, for the purpose of cleansing and purifying suppurating wounds and ulcers, it causes pain, does not favour granulation and the development of skin, and has an unpleasant odour, so that he has often been obliged to resort to other measures, as to chlorinated soda and permanganate of potash. It deserves however, he thinks, extensive employment when very diluted (in the proportion of 1 to 2 drachms to a pint of water), as a means of preventing contagion. He prefers the employment of sulphate of copper, zinc, acetate of lead, tannin, alum, &c., to carbolic acid in cases of gonorrhœa as an injection, which, he thinks, may prove absolutely injurious. (*Arztliches Literaturblatt*, April 1872.)

Therapeutic Value of Tannate of Quinine.—M. Sistach observes that the tannate of quinine is, when administered in equal doses, much less active than the sulphate, but when given in larger doses it has the same therapeutic value, and even presents certain advantages which render it preferable for nervous subjects. M. Hérard has also communicated to the Académie de Médecine a note from M. Lambon (de Bagnères), containing the results of the experiments he had been commissioned by the Academy to make on the effects of tannate of quinine in ague. The conclusions at which he had arrived were:—1. That the tannate of quinine readily and completely

cures intermittent fever : it must, however, be given in somewhat larger doses than the sulphate of quinine. (Sulphate of quinine contains one atom of quinine and one atom of sulphuric acid, whereas the tannate contains two atoms of tannic acid to one of quinine.) 2. In giving tannate of quinine in double quantities as compared with sulphate of quinine, nearly the same effects are produced. 3. Under these conditions the spleen may be observed to diminish in size, though rather more slowly than with the sulphate. 4. The physiological action of the tannate is less rapid than that of the sulphate; therefore, in the case of *pernicious* intermittent fever, he considers it wise to prefer the tannate of quinine. 5. The tannate is less irritating to the stomach, and especially to the peptic glands; it is less injurious to digestion, and is not so soon tired of; it can consequently be prescribed for a longer period, which is essential for the cure of rebellious intermittent fevers and the return of the spleen to its natural size, which constitutes the almost certain indication of a real cure. 6. When intermittent fever is accompanied by diarrhoea, the tannate ought to be preferred to the sulphate, since this improves the intestinal condition, whilst the sulphate often increases the symptoms. 7. When the febrile attack ends in profuse sweating, the tannate acts much more beneficially than the sulphate. This observation has led M. Lambron to prescribe the tannate of quinine, often with advantage, in the sweats of phthisis. (*Journal de Médecine et de Chirurgie*, Avril 1872.)

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[This research will be noticed in our next number.]

¹ Any of the foreign works may be procured on application to Messrs. Dulau, of Soho Square, W.C.; Williams & Norgate, of Henrietta Street, Covent Garden, W.C.; or Baillière, of King William Street, Charing Cross.

THE PRACTITIONER.

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TWO CASES OF IMPAIRED WRITING POWER TREATED SUCCESSFULLY BY ELECTRICITY.

BY THOMAS BUZZARD, M.D.,

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I HAVE preferred employing the general title which heads this article to one more definite, because, although in each case the patient applied to me on account of the difficulty he experienced in using his pen, the cause of that difficulty was entirely different in the two instances. To the first case the term "Writer's Cramp" would be fairly applicable; in the second no spasmodic affection was present, but the patient was suffering from paralysis of the ulnar nerve with consequent atrophy of the muscles supplied by it. The second patient was a friend of the first, and was sent to me by him because of the success which had attended the treatment of his own ailment. Although in both instances electricity was employed with advantage, yet the form and mode of employment entirely differed. I have thought, however, that in spite of the pathological contrast the two cases might well be related together, on account of the characteristic symptom—impairment of writing power—which

in each instance formed the chief source of the patient's trouble, and because an opportunity is thereby afforded of illustrating some important and widely differing uses of electricity.

CASE I.—A. B., aged 27, was sent to me in February 1869 by my friend Mr. Nunn. The patient was a healthy-looking gentleman, whose occupation had at times entailed a good deal of writing. For two or three years he had noticed that his right arm was liable to get numbed and stiff after using his pen, and during the last year there had been a constant tendency of the hand to roll over outwards as soon as he began to write. This had increased so much that now, whilst simply signing his name, the words could not be finished ere the pen was lifted from the paper on account of his hand having gradually passed into a position of supination. The difficulty was not confined to the act of writing, but he experienced it in various movements, such as driving, lifting objects, shaving, dealing cards. Even in the act of shaking hands he felt great awkwardness. The ailment was sufficient to produce great and constant inconvenience; he could not write a letter, and the mere act of signing his name to a cheque was accomplished with great effort. He succeeded best by raising the elbow and resting the wrist alone upon the left hand. Occasionally he had cramp also in the fingers, which twisted and interlaced so as to render his pen useless.

Family history.—A brother of his mother died in a fit; a brother of his father from heart-disease. An uncle suffers much from nervousness.

Personal history.—He has been married six months. He is well nourished, of active habits, accustomed to hunt a good deal, and to ride daily. His general health has always been good with the exception of "bilious attacks" and indigestion. He is naturally of a nervous temperament.

Five or six years ago, in jumping his horse he fell and struck the back of his neck. He felt some inconvenience for a day or two, but not afterwards. When a boy he used to hold his pen with undue tightness. A writing-master cautioned him against this, and said that "the day would come when he would not be able to hold his pen at all."

I advised that he should give up writing, should keep his arm in a splint for six hours daily, and should take three minims of

the liquor arsenicalis twice a day. On March 23rd the dose was increased to four minims.

April 13th.—A suggestion of Mr. Nunn that there might possibly be congenital absence or atrophy of the pronator radii teres caused me to test the muscles of the fore-arm with the induced current. I found, however, that they all responded readily, and that the movement of pronation was well performed when the pronator teres was acted upon by the current, and that there was no apparent wasting of the arm. In view of the spasmodic action of the supinators, I advised that the antagonistic muscles, the pronator and the flexors, should be faradised three times a week—that the dumb-bell should be employed in movements of pronation only, and that the rest in a splint should be continued for six hours daily. Writing was still to be avoided. An experiment that he made at my desk showed that the spasmodic action was as bad as ever. He said that his general health was improved, that he suffered but little now from nervousness or indigestion, and that he had gained four pounds weight since last seen.

The treatment recommended was sedulously carried out by Mr. R. Bond Moore, of Wolverhampton, the patient's private medical adviser. I am indebted to Mr. Moore for notes of the measurements of the arm from time to time. In the beginning of May the fore-arm, at its thickest part, measured 10 inches; on May 24th, $10\frac{1}{4}$ inches; June 19th, $10\frac{3}{8}$ inches. On July 3rd, Mr. Moore found his cramp more marked, and the measurement at the same point was only 10 inches. The patient had been staying at Llandudno for a fortnight, during which the Faradism had been omitted. The arm had returned to its original circumference of May 3rd. The Faradism was resumed, and when I saw him on July 9th there was some improvement. His treatment was continued. On August 7th the arm measured $10\frac{1}{4}$ inches; on September 13th, $10\frac{1}{2}$ inches; on October 11th, $10\frac{1}{2}$ inches; November 20th, $10\frac{3}{8}$ inches; January 30th, 1870, $10\frac{3}{4}$ inches.

During this time the electrical treatment had been continued three times weekly, and the arsenic continued to be taken. There was a gradual improvement in the co-ordinating power of the arm, which progressed steadily. When I saw him on

November 5th, 1870, I made the following note:—"He can write now without inconvenience. The hand will sometimes incline to roll over, but never entirely does so. He can shave himself now, and carve at table perfectly well, which he could not do formerly. He can deal cards now with ease."

The electrical treatment was continued, though now at longer intervals, determined by the return of any tendency of the hand to roll over or feeling of weakness.

June 19th, 1872.—He came up to town to see me. He has used his arm much more for writing during the past year; and during the last two months, owing to the illness of his partner, he has employed it very considerably. *The hand never turns over* now, and the only notable point is that after more than usual work his arm will feel somewhat tired. No electrical treatment has been applied for two months. He wrote freely in my room, and nothing abnormal was to be observed in the movement of the hand.

On July 12th, 1872, Mr. Moore found the right arm to measure $10\frac{3}{4}$ inches at the point at which in May 1869 it measured 10 inches.

This was an instance of writer's cramp in not perhaps its most common form, for it is more usual to find the functional spasm affecting one or other of the muscles immediately concerned in *holding* the pen, and showing itself by cramp in the thumb, index, or middle finger. In this case the act of writing immediately began to call forth apparently a state of tonic spasm of one or more of the muscles which supinate the hand. This condition was inferred from the action, as nothing abnormal could be felt in the arm whilst the hand rolled over. The local treatment consisted partly in long periods of rest to the arm generally, and partly in developing to the utmost the muscles antagonistic to the act of supination—the flexors and pronators. The result has been highly satisfactory, and it seems likely that, provided the patient continues to avoid great excess of writing, he may remain free from the distressing difficulty which he used to experience. It is interesting to note that the development of the size of the fore-arm in response to the Faradism was accompanied by diminution of cramp, whereas, when the electrical treatment was suspended from any cause, the arm grew smaller

and the cramp returned. The patient himself thought that he derived great benefit from the arsenic, which he took for fifteen months. He found that under its use the "bilious attacks" and indigestion from which he had been apt to suffer almost entirely disappeared. No doubt these attacks were of nervous origin; both in his family and personally there is a neurotic history. Obscure as is the pathology of such cases, one would be disposed to consider his local ailment as being due primarily to a central cause—a peculiar condition of nerve-cells constituting a centre of co-ordination. A neurotic history, either personal or in the patient's family, is always to be found, according to my experience, in cases of "writer's cramp." It would be very unwise, however, to infer from this the inutility of localised treatment, which is, of course, often of important service in many affections which owe their origin to central mischief. Dr. Anstie has illustrated this remarkably in the case of neuralgia, and there is no lack of examples in numerous other diseases.

The case which I have related seems worthy of being placed upon record, because so successful a result is exceptional. One great cause of this is, I believe, that sufferers as a rule are not possessed of sufficient perseverance to submit to the long and persistent treatment which is requisite in such cases. In the present instance, the intelligence of our patient, and his conscientious submission to the scheme of treatment devised for him, had doubtless much to do with his recovery.

CASE II.—C. D., aged 45, applied to me on March 7th, 1872, on account of want of power in his right hand. About two months before he had noticed that his writing became very bad, from the ring and little finger failing to give proper support to his hand as it held the pen. This difficulty increased, and was soon accompanied by inability to hold the pen firmly between the thumb and first two fingers. To obviate this he would use a short pencil, the end of which was planted in the palm, whilst its axis was grasped by all the fingers, and he wrote without leaning upon the two inner ones, but sometimes used the extremity of the ulna as a support. He could not cut a pencil in the ordinary way, nor could he snap the index or middle finger against the thumb.

For some years past he has had much brain fag, and early in

last winter he broke down in health so much that he obtained medical treatment, under which he had greatly improved. During the winter he had suffered much from pains in various parts of the right shoulder and arm. First there was an obscure pain about the inferior angle of the scapula, with a dull, tired feeling down the right side of the back. This has continued ever since when he uses the arm at all freely. In this case it is apt to run down the arm and localise itself about the external condyle of the humerus, where there is still a tender spot on pressure. The next symptom was that the ring and little fingers could not voluntarily be brought together. About a week afterwards he got acute pain about the lower end of the ulna and along the ulnar side of the palm; and there is still a tender spot just between the heads of the abductor and short flexor of the little finger. In the clefts between the first three metacarpal bones he would get pain, which generally occurred at night, and was sufficient to wake him. It was increased by movement, so that he had to lie at night with his palm upon his chest and the arm restrained by a sling. With this, too, he had pain about the shoulder-joint, and a tender spot is still to be found in the neighbourhood of the circumflex nerve. After writing long he feels uneasiness along the axilla and inner border of the biceps, and a sensation of brain distress like that which would accompany inaffectual efforts to pick up a small object with the toes.

A glance at the hand showed that it was wasted, and a more minute examination allotted the atrophy to the muscles supplied by the ulnar nerve—notably to the dorsal interossei and the muscles constituting the hypothenar eminence. There was also some falling away of the plump of the thumb, corresponding doubtless to atrophy of the adductor pollicis. There was no marked abduction of the hand nor wasting of the fore-arm. The thumb could not be brought with any firmness against the fore finger. The ring and little fingers tended to remain semiflexed upon their metacarpal bones. The little finger stood out from the rest, and could not by the strongest effort of the will be brought towards its neighbour. Neither the ring nor the middle finger could be moved voluntarily in either lateral direction. The index could be moved slightly from side to side.

There was no definite loss of cutaneous sensibility in the hand. The fingers could be well flexed upon the palm, but extension was imperfect, especially in the third and fourth.

Electrical examination.—To the strongest power of a one-celled Stöhrer's induced current battery (Faradism) there was no response whatever in any muscle of the little finger, in any of the interossei, in the adductor pollicis, or in the flexor carpi ulnaris. The flexor profundus digitorum was out of reach of this test.

Next Foveaux's portable constant current battery was employed, with a strength of thirty-eight cells. The rheophores (of metal, leather-covered, and moist with salt-water) were applied about half an inch apart to the clefts between the metacarpal bones, where the dorsal interossei can be got at. (The clefts, I should say, were marked hollows, owing to the muscular atrophy.) By labile application (interruptions every second or so) there was brisk action of all the interossei, the index and ring fingers being drawn away severally from the middle finger, and this last drawn to the radial or ulnar side, according as the second or third dorsal interosseus was stimulated.

The phenomena pointing to lesion of the ulnar nerve, I inquired particularly regarding any injury. It appeared that previous to the appearance of the weakness in the hand, the patient had been writing day after day for long hours, wearing a thick coat which was painfully tight at the upper part of the arm—so tight, indeed, that he had been obliged to send it away to be altered.

Treatment.—Guided by the results of the electrical exploration, I decided upon employing for the present the constant current for provoking contractions of the affected muscles, and thereby improving their nutrition, hoping in time to substitute Faradism for it as its power declined. My patient was fortunately an unusually intelligent man, and he has greatly to thank his own patient submission to treatment for the results obtained.

I do not intend to enter upon any daily record of the electrical applications, but to give a sketch of the progress of the case under this treatment.

Electricity was applied three times a week, the sitting generally occupying nearly an hour. The constant current was usually

applied as I have described, but occasionally I placed the positive pole upon the mid-cervical region, and the negative with the hand in water for a few minutes, ten cells being employed. On other occasions the positive pole was applied to the elbow where the ulnar nerve is superficial, and then, the negative being immersed with the hand in water, the current from twenty cells was used. I never failed besides to test all the affected muscles with Faradism. On March 30th Faradism gave good contractions in the flexor carpi ulnaris, but not equal to those of the corresponding muscle on the left. On April 6th Faradism excited slight contractions of the adductor pollicis and first dorsal interossei. Two days after this fifty cells of the constant current failed to excite contraction in the first dorsal interossei, but Faradism (of rather high power) here gave marked result. About this time the patient complained of pain lasting some hours at the points of application of the electricity. The wasting of the hand became remarkably diminished. On May 17th the primary current of Stöhrer's battery gave marked contractions in the little finger muscles and first interossei, but not in the other interossei, in which, however, from thirty to forty cells of the constant current continued to excite contractions. On the 30th May he could bring the little finger very near to the ring finger by a voluntary effort, which, however, caused a painful sense of exhaustion. A week later distinct contractions responded to Faradism applied in the third dorsal interosseous space. By the end of June the nutrition of the hand had so far improved that an uninitiated observer would have failed to notice anything abnormal in it. On July 11th I discontinued all treatment, as he was quite cured. The hand was still perhaps not absolutely so strong as it should be, but every movement of the fingers could be perfectly performed, and all sign of wasting had disappeared; yet the *electrical* contraction of the hypothenar muscles failed to move the little finger, which readily answered to the stimulus of the will. He was again able to write in the ordinary manner. In round numbers his ailment lasted six months, during the latter four of which he was under treatment. Dr. Down saw the patient with me in consultation in May, when the excitability to Faradism was beginning to be well marked.

This case manifestly deals with a train of phenomena perfectly different to those of the first. If I am right in my diagnosis, we

have here associated general impaired nerve nutrition—lesion of a single mixed nerve from pressure—consequent paralysis and atrophy of muscles supplied by it—repair of the injury to this nerve-trunk by time, aided perhaps by the constant current—preservation of the atrophied muscles from irreparable degeneration, and stimulus to their growth by electrical treatment adapted to their varying condition. So far as I know, it is impossible to speak with any certainty of the exact nature of the lesion of the ulnar nerve; but perhaps, considering the pain and tenderness in parts of its distribution, we should be warranted in considering it as neuritis provoked by pressure upon a feebly nourished nerve-trunk. The diagnosis of the case offered points of great interest. The sudden appearance of atrophy in the cleft between the thumb and fore-finger, in a patient broken down generally in his nervous system, suggested the possibility of the commencement of progressive muscular atrophy. Some strength was added to this by the fact that the patient would complain occasionally of pain in the corresponding part of the *left* hand as well as in the right; but this symptom occurred after prolonged electrical applications, and I explain it as a reflex phenomenon. It has long entirely ceased. I judged his local ailment to depend upon peripheral lesion of the ulnar nerve chiefly for two reasons. One was that the muscles affected corresponded exactly with branches of that nerve alone, and we know that in progressive muscular atrophy muscles are prone to waste which are not associated in their nervous supply. The other was, that in this latter disease the electrical condition of atrophied muscles generally remains normal; they respond, so far as their diminished bulk permits, to Faradism, and are unaffected by the labile constant current. As has been noted, this was not the case with this patient. The electrical condition was precisely that which, *e.g.*, obtains so often in the muscles of the face when the facial nerve has sustained a peripheral lesion by cold, pressure, or violent injury. The electrical exploration was thus of most important service, and enabled me, with the help of the history of the tight-sleeved coat, to explain the phenomena, and promise, with some confidence, a restoration of function.

As regards the pains experienced by this patient, it will have been remarked that many of these were remote in their situation from the distribution of the ulnar nerve. Without attempting

to explain this phenomenon, I would only remark that a similar circumstance has been noted, as occurring after injuries of nerves, by Messrs. Mitchell, Morehouse, and Keen, in their admirable work.¹ I may add, that although pain is not a usual concomitant of progressive muscular atrophy, I have frequently known it occur antecedently to the wasting of muscle in that disease, ceasing when the atrophy became confirmed. The existence, therefore, of this symptom did not help me in my diagnosis of this case.

In muscles and nerves which are completely paralysed we frequently find, as is well known, that the excitability for the intermitted battery current is retained or even augmented, whilst the excitability for induced currents is completely lost. In such cases the reparative action of the continuous current is greatly superior to that of the induced. With the return of motility the excitability for the different currents is usually reversed. This was well illustrated in the case described. It seems likely that the explanation of this phenomenon offered by Neumann and Brückner is the most feasible. Their experiments go to show that it is a question of duration and greater or less rapidity of succession. If the battery (or continuous) current be intermitted with extreme rapidity, it ceases to possess the contractile power which marked it when the intermissions were slower and more prolonged. It would seem that a stronger momentary current produces less effect than the interruption and completion of a weaker one of longer duration.

I remarked that when this patient was cured, and could move his little finger voluntarily in all directions, the induced current applied to the muscles caused contractions which, though perfectly evident to the eye, failed to move the finger. It is probable, I think, that this depended upon the amount of muscular tissue thrown into contraction by the electrical stimulus being less than that which the will, through the intra-muscular nerves, was able to affect, owing possibly to a failure of tonicity in the muscle. Another point of interest was the fact that, to the last, the second and fourth interossei remained insensitive to the induced current, although volition contracted them freely. A similar condition has been noted by Duchenne and Ziemssen amongst others.

¹ "Gunshot Wounds and other Injuries of Nerves." Philadelphia, 1864.

ON THORACENTESIS.

BY DR. CLIFFORD ALLBUTT.

THERE is no doubt that the imagination often influences us largely in our choice of operations, and that we readily undertake some operations which are desperately dangerous, with light hearts, because there is nothing alarming in them to the imagination, while others which do exercise some potent spell over the imagination are really harmless enough, and may be undertaken almost without risk. Any interference with "vital parts" is awful to contemplate, and thus hundreds of persons have died of excessive effusion in acute pericarditis without an attempt to save them; but I have shown that the pericardium may be tapped in acute pericarditis with safety and success, the operation being among the simpler feats of surgery. There was nothing to cause this dread of the operation, save the sense of awful vitality belonging to the heart; the fact being, as any experimental physiologist knows, that the heart will stand a degree of interference far beyond anything required in this simple operation. The same glamour was thrown in a less degree over the whole chest, so that thoracentesis in pleuritic effusion has only become common within quite recent times, and is now far from being popular. I propose in this paper to add to the valuable records which have been published on this subject, and to strengthen the hands of those who have to deal with large intra-thoracic effusions. During the last three years I have advised thoracentesis in fourteen cases, and with but one doubtful result. In many cases as much as one hundred or one hundred and twenty ounces were drawn off. Indeed the one

doubtful case was doubtful only in appearance, and so far as the operation was concerned, it may justly be reckoned among the successful ones.

A. B. was a man of phthisical aspect, who was admitted into the Leeds Infirmary suffering from hydrothorax on the left side. There was no history of any acute symptoms. We drew off about seventy ounces of fluid from the chest, with temporary relief. The man rose from bed, regained some facility in breathing, and the lung seemed, in a measure, to expand. He did not regain either strength, flesh, or appetite, and he had a rather high temperature, night sweats, and cough. The state of the left lung was not easy to determine; over the right apex there were some slight signs of degenerative mischief. He left the hospital in about three weeks after the operation, relieved in breathing, and somewhat better in his general health. The fluid did not return while he was under our care. Here it is likely that we had to deal either with a case of phthisis with that kind of latent hydrothorax which we often find in such patients, or with a case in which long-continued compression of a lung had brought about a phthisical state of that lung, and soon afterwards of the other. We seem to have carelessly postponed a thorough examination of the right lung until our suspicions were aroused by the man's imperfect convalescence. The effusion was not discovered until his admission; its duration was therefore unknown. It is quite possible that in this man's case the phthisis was independent, or at any rate no consequence of the effusion; but I am disposed to think otherwise. The man had a good family history: he had reached the age of thirty-five, and had had good health until he began to suffer from tight breathing. Be this as it may, I do but use the case as an occasion for introducing a statement of my belief that such a sequence—namely, phthisical degeneration, fibroid or other, as a result of long-continued compression—is far from uncommon, and this chance is to my mind one of the strongest arguments in favour of early thoracentesis. In clinical teaching I always urge early puncture, on the following grounds:—

1. That the compression of the lung, if long continued, is likely so to injure the nutrition of the lung as to make it liable to degenerative change; or at any rate the lung is likely to become

so bound down and crushed as to be unable ever to recover its normal functions.

2. That the presence of a large quantity of fluid in the chest, especially when on the left side, as it so commonly is, is liable to embarrass the heart so seriously as to put the patient in jeopardy of his life; and that, in fact, death by syncope in hydrothorax is far from uncommon.

3. That the fluid pressure brings about gradual abolition of the absorptive power of the pleura.

4. That there is a fear lest the sero-fibrous exsudate may become purulent.

These contingencies suffice to make us very anxious until we get rid of the fluid by some means.

Now, have we any real ground for believing that we can do this with anything like *promptitude* with medicine? I think no cautious practitioner will assert that we can. If I find any large amount of fluid in the pleura of a patient of my own, I now never try medicine at all, unless the friends or the family attendant urge such a course; for I find in my records of cases treated by drugs, and in such records as I have of such cases so treated by others, the results are very very far inferior to those in which puncture was employed. It is but the other day that a case came under my notice of a poor gentleman who had gone about for many months with his left pleura nearly full of fluid, and who was treated with mercurials, digitalis, iodides, tonics, and good nutrients, but in vain. In the end the fluid subsided in considerable measure, but chronic pneumonia supervened in the injured lung, and he ultimately died. I have no reasonable doubt that his life could have been spared, and his health quickly restored, had he been tapped at the beginning.¹

I have now a gentleman under my care for empyema, who has had the "most judicious" drug treatment for left hydrothorax; but the hydrothorax drifted into empyema; the pus had burst through the lung when I saw him, and he was rapidly failing. I still, as my wont in such cases is, urged thoracentesis: this

¹ I may refer to the case of a lady, a hale woman and mother of a large family, who was treated for eighteen months by drugs and change of climate for pleuritic effusion. Slowly the effusion gave way, but fibroid degeneration took possession of the crushed lung, and she died a few weeks ago.

was done; a drainage tube was inserted, the discharge through the lung gradually ceased, and the patient will probably get nearly well. I judge from the evidence of post-mortem examination, when we see the walls of the pleural cavity thickened and inert, that to try to remove fluid from its interior by the use of diuretics is much as though we tried to empty a cask by pouring water over the outside of it. I believe that in the majority of cases in which there is much effusion, or in which the effusion is stationary, the result of treatment by drugs will be found to be disappointing, and to stand at great disadvantage when compared with the results of thoracentesis. At the same time, when the fluid is small in quantity, when there is no pressure upon the heart, when there is probably no great crushing of lung, and no soddening and spoiling of the pleura, then I think the drug and blister treatment is both indicated and often successful. There is, however, an exception to this rule, if it be an exception, and this is, that when the effusion is bilateral, or when there is fluid also in the pericardium, puncture should be practised at once. A remarkable instance of this occurred about eighteen months ago in the case of a gentleman under the care of Mr. Smith, of Halifax, and Mr. Joseph Teale, of Leeds, and to whom I was called by those gentlemen. He was suffering from acute rheumatism, and had effusion into two-thirds of the left pleural cavity, one-third of the right cavity, and into the pericardium. Death was imminent. We first determined to tap either the left pleura or the pericardium, and finally we resolved upon the left pleura. Mr. Smith drew off some twenty ounces of highly fibrinous exsudate, with the result of setting up rapid absorption, not only in this cavity but in the other two cavities also, and we had that unspeakable satisfaction of snatching a fellow-creature from the edge of the grave which cheers us in the practice of our art. I may add that, owing probably to the mischance of a small adhesion, the lung was pricked in this case; air welled quickly forth, and the dyspnoea was aggravated for a few minutes to a desperate point; but as this danger passed off, no other ill consequences appeared. One more example also I found in this case of a practice which I have long urged, paradoxical as it may appear, which is, that in cases such as the one mentioned, where the effusion is likely

to be highly fibrinous, we should not use a large trocar, but on the contrary the smallest that can be obtained.¹ I have always noticed that it is not the small trocars, but the large ones, which get blocked. The ordinary-sized Bowditch canula is rather too large for highly fibrinous effusions. As this is but a desultory paper, I may allow myself now to run on quickly to consider the alleged drawbacks to early thoracentesis.

1. The fear of operation and the pain of it. This may be overcome partly by reason, and partly, if desirable, by freezing the cuticle to be punctured.

2. The fear of reaccumulation. This is a bugbear—a bugbear, that is, in all cases where the plea can be relevant. My own cases of thoracentesis have been numerous, and to these I have to add many that have come under my observation in the practice of my colleagues and friends, and I may safely say that it is not the acute or “inflammatory” effusions which re-accumulate. Even the drawing off of a small quantity (which is desirable in case the withdrawal cause paroxysmal cough or faintness) usually ends not in re-accumulation, but in resorption. To say that the fluid reappears in the hydrothorax of Bright’s disease, of heart-disease, and the like, is true enough, but what of that? About ten years ago I consented to the death of a poor lady who had Bright’s disease, and therewith hydrothorax in both pleuræ, and her life ebbed as the tides rose in the chest. It was urged that to tap such a patient was bad practice, as its reappearance was certain, and her complaint surely fatal. I have always thought that we three doctors did on that occasion shirk our duty—which, I take it, is to prolong life to the uttermost. Subsequent experience has taught me that in such cases tapping is often of the greatest value both as a means of immediate relief and of indefinite palliation. Many days, or even weeks, elapse before the lungs are again seriously embarrassed, and then we have but to tap again. In one case of this kind, after the third tapping the fluid no longer persisted in its course, and we obtained a clear respite of some six months. In these cases I believe tapping can be done the more easily, as

¹ A plan of “capillary thoracentesis” was recommended by M. Blachez in the *Union Médicale* three or four years ago, but I have no knowledge of the operation beyond his statements.

there is the less to be feared from entrance of air. The lung is not so liable to be bound down, and the pleuræ seem indisposed towards pyogenesis, the fluid being but slightly fibrinous and containing few or no leucocytes. On the other hand, to take the next objection to thoracentesis, I do, in opposition to some of my friends whose opinion is most weighty, hold that in cases of sub-inflammatory hydrothorax there is a great tendency to pyogenesis unless all air be excluded. In highly inflammatory cases, such as the acute rheumatic, there does not seem to be much risk of turning the fluid into pus; but in the sub-inflammatory, cachectic, and latent forms, in which also tapping is more often required—in these the tapping is more likely to be followed by empyema, as the tendency to pyoid conditions is stronger. Some little time ago I had the pericardium of a girl tapped in a case of this kind. Mr. Teale drew off some eight or nine ounces of quite clear fluid, containing a little fibrin on standing, and a few leucocytes. We made no attempt to exclude air, and air freely entered the pericardium. In about two days the pericardium refilled, and we drew off the same quantity of fluid, but now it was distinctly purulent even to the naked eye.¹ And the cases of hydrothorax in which I have noticed the same result were of this kind—not the simply dropsical, nor the highly inflammatory, but the intermediate sub-inflammatory or “cachectic” forms. In these cases I am sure air should be excluded most rigorously.

P.S.—While correcting this proof, Dr. Maclagan’s remarks on the same subject in the *British Medical Journal* for July 20th come into my hands. It will be seen that I think pyoid degeneration of the effusion depends less upon the admission of “germs” and more upon oxidation of certain low forms of effusion. Highly plastic effusions on the one hand, and dropsical effusions on the other, rarely become purulent on admission of air. As to the non-expansion of the lung, I believe it is not needful in serous effusion to draw off more fluid than the re-expansion of the lung will compensate; renewed absorption will carry away the remainder. We find the new aspirator answers very well at Leeds.

¹ The poor girl died shortly after, from pre-existing broncho-pneumonia.

TWO CASES ILLUSTRATING THE TREATMENT OF CHRONIC GLAUCOMA.

UNDER THE CARE OF MR. W. SPENCER WATSON.

CASE I.

(Royal South London Ophthalmic Hospital.)

ATROPHY OF THE OPTIC NERVES WITH EXCAVATION—IRIDECTOMY —PARTIAL RECOVERY OF SIGHT.

JOSEPH H., æt. thirty-eight years, a working silversmith, was admitted on April 18th, 1871, under the following circumstances:—

He is married; has five children, one of whom is subject to fits. He himself enjoyed good health and good sight till one year and nine months before admission, when he had severe neuralgia in the right side of the head, extending subsequently to the lumbar region and even to the soles of the feet. At this time his sight began to fail, and when admitted he was so nearly blind that he was led into the room.

On testing his vision, it was found that he had mere perception of light with the right eye, and with the left could just distinguish the fingers of a hand held between him and the light.

When examined by the ophthalmoscope, the characteristic pearly whiteness of the optic discs, with a deep excavation in each, led to a diagnosis of atrophy. A slight degree of glaucomatous tension ($T\frac{1}{2}$) made it appear probable that some benefit might be obtained by iridectomy, which was proposed, a very unfavourable prognosis being at the same time expressed. The operation was performed on May 5th, 1871, on both eyes, about one-fifth of the iris being removed from its upper circum-

ference in each. There was an almost immediate improvement in the sight of each eye until May 9th, when he was accidentally struck in the right eye, the anterior chamber of which became filled with blood.

May 19th.—In both eyes tension normal: in the Right, vision slightly improved; Left eye, vision, letters of No. 16 of Jäger's test-types distinguished; the field of vision extended.

Since this date vision has continued to improve so much in the left eye that on January 9th, 1872, he had been at his work as a silversmith for three or four weeks; vision of left, Jäger No. 16 without glasses, and Jäger No. 6 with a convex lens of ten-inch focal length.

On February 9th, 1872, the field of vision of the left eye was still limited, being represented by a diagram with an area of nine inches in the upward and inward directions, one inch downwards and two and a-half inches outwards. Reads Jäger No. 16. With the right eye vision still the same; he cannot tell a finger from a thumb, nor read even the largest type. On ophthalmoscopic examination the optic discs are still of a pearly-white colour and somewhat excavated.

May 30th.—The sight seems to have deteriorated slightly. He was therefore ordered liq. strychniæ ℥v., to be taken three times daily.

June 10th.—After taking the strychnia for a week he began to have muscular "twitchings" and a slight stiffness of the jaw, but thought his sight was improving.

Remarks.—The case is one of a numerous class generally considered hopeless, and in which it is always necessary to give a most cautious prognosis. The result of the operation was, however, more favourable than could have been expected, and it is probable that had not the right eye suffered from the accident a few days after the iridectomy, the sight in this eye might have been restored to as great an extent as in the left.

The remarkable improvement in the sight of the left eye amply justifies iridectomy in any similar case, even when some considerable time has elapsed after the acute symptoms. For it is probable that the attack of so-called neuralgia with which the failure of vision was ushered in was really one of acute glaucoma.

The patient was a man of very abstemious habits, and had been a total abstainer till about two years before his failure of sight commenced.

July 20th.—He has continued the liq. strychniæ since the last note, and now, with spectacles (bi-convex of fourteen-inch focal length), can with some difficulty read the leading articles of a newspaper.

CASE II.

(Great Northern Hospital.)

SUBACUTE SUPERVENING ON CHRONIC GLAUCOMA—TREATMENT BY IRIDECTOMY—GOOD RESULTS.

MRS. B., æt. fifty years, having been for several weeks under treatment by Dr. Cholmeley for chronic bronchitis, was transferred to Mr. Watson's care in consequence of subacute pains and congestion of the eyes, with marked impairment of vision, increased tension, dilated pupils, and excavated optic discs. A year ago she had noticed the appearance of rainbows round the candle, and her sight had been failing ever since.

On December 15th, 1871, the sight of the left eye was so impaired that she could only just count fingers held up close to her eye. With the right she could read words of No. 18 of Jäger's test-types. The symptoms had only been going on for six weeks in the right eye.

Iridectomy upwards was performed in both eyes.

The result is that on May 10th the sight had been improving steadily, and she could read No. 12 Jäger with the right, and No. 14 with the left eye. It was found that, in consequence of a great degree of presbyopia, a bi-convex glass of ten-inch focal length was of great service, and with spectacles of this strength she read much smaller type.

On June 27th, she states that she can read large print and do a little needlework. She has never had pain in her eyes since the operation. The ophthalmoscope still shows excavation of the optic discs and extreme pallor, very closely resembling that characteristic of atrophy.

THE ELIMINATION THEORY OF THE CURE OF DISEASES.

BY DR. ANSTIE.

PART IV.

ELIMINATION OF ALKALOIDAL POISONS.

IN former papers we have investigated the action of the organism when poisoned with narcotic doses of alcohol, and its behaviour in presence of mineral poisons. In neither case did we find the least reason to believe that there was such an escape of the poisonous matters as could produce any considerable remedial effect. In the present article we shall examine a different class of poisons, viz. the alkaloids, in order to see whether they, by their manner of elimination, afford any analogical support to the theory of Nature's cure of diseases by the expulsion of morbid matters from the body.

One of the most striking examples of a dangerous alkaloidal poison that will occur to everyone's mind is strychnia, an agent about which a great deal, though not everything, is known with fair certainty. To state compendiously the general effect of the researches that have been made so far, we may say that strychnia appears to stand in very much the same position as arsenic and antimony, with regard to elimination. It diffuses rapidly into all parts of the body. A portion undoubtedly escapes in the urine; this was long ago stated by Macadam, Hamilton, Masing, and others, and can be pretty easily verified (as I have done for my own satisfaction when lecturing on Toxicology) by extracting the filtered urine of strychnia-poisoned dogs with chloroform or benzol. Strychnia was found, by Gay,

to escape in small quantities with the saliva. It has also been shown, by various observers, that strychnia remains deposited (unless it has been given in a dose which is exactly or nearly the minimum that will kill) in the liver, the kidneys, the spleen, the pancreas, the muscles, and various parts of the central nervous system of animals which have died from its effects: this has been demonstrated in England by Macadam, Anderson, Rodgers and Girdwood, by Wrightson, Herapath, &c.; and on the Continent by Dragendorff and Masing, by Gay and others. Here, then, we have again, as in the case of arsenic, all the *primâ facie* appearances of a purely physical distribution of the poison, of which distribution the escape of a portion in the urine and saliva is merely a casual and limited feature. It is necessary, however, to study carefully the question, what is the *rationale* of the occasional recoveries that take place? for there are a certain number of recorded instances of recovery even from doses much larger than those which usually prove fatal. Thus Hamilton recorded a recovery after a dose of four grains, Williams one after five grains, Tschepke¹ one after twenty-two grains; and several other cases are recorded in which very massive doses failed to kill. Now, the following points deserve special notice. In the first place, the number of recoveries from anything like a substantial toxic dose of strychnia is very limited relatively to such occurrences in the case of many other, even formidable, poisons: I have been struck with this in looking over the now very extensive literature of strychnia-poisoning. Secondly, among this limited number of recoveries there appear to be quite as many, if not more, from doses which greatly exceeded the normal line of fatality than from smaller doses. Thirdly (and this is important), I can state, from my own experience, that any irregularity as to the fatal action of strychnia, when given in certain quantities, vanishes completely if we adopt a particular method of administration. From the results of a large number of experiments which I made on cats, dogs, rats, and frogs,² I can affirm that if strychnia be injected

¹ This, however, was a complicated case: there were morphia and laurel water mixed with the strychnia.

² These experiments were preliminary to some special investigations which appear in "Stimulants and Narcotics."

into the great cavities of the body, in a tolerably dilute state, the results are almost absolutely uniform: death always occurs with great rapidity; and the exact degree of rapidity and of the violence of symptoms is apparently governed merely by the species of animal, the body-weight, the dose, and the dilution. This immediately suggests the idea that the occasional recoveries from large doses of strychnia taken by the mouth may have been due merely to imperfect absorption, a portion of the poison having altogether failed to enter the circulation. Accordingly we look about us for other instances in which strychnia has failed to produce a fatal degree of toxic action: and a series of recently observed facts immediately occurs to the mind. It has been discovered by Mr. Barwell that large doses of strychnia can be injected into the muscular tissue without any visible production of toxic effects, provided that the solution be *sufficiently concentrated*. The same, or much smaller quantities, injected in a dilute solution, would undoubtedly produce grave or even fatal tetanus: and the whole secret of immunity appears to lie in the fact that concentrated solutions irritate the tissues locally, and thus become encysted and confined to one portion of the extra-vascular area. It seems not merely probable, but almost certain, that a similar irritant quality of large doses of strychnia, swallowed by the mouth, prevents their full absorption through the mucous membrane of the alimentary canal; aided, possibly, by the decomposing effect of gastric and intestinal secretions which are in a peculiar chemical condition.

In any case it must be allowed, I think, that the general phenomena of acute poisoning with strychnia are far from indicating the existence of any effective power of the organism to defend itself against strychnia by means of elimination. It may be said, without serious inaccuracy, that unless hindered by some rare and accidental circumstance (such as the irritant effects induced by over-concentration, or the occurrence of spontaneous or of artificial vomiting very soon after the poison has been swallowed), strychnia in doses of from $\frac{3}{4}$ to 2 grains will *always* kill an adult human being in from half an hour to three or four hours: and there is no reason to think that the escape of the poison in the urine and saliva affects the issue in the least degree.

There is a particular kind of *chronic* strychnia-poisoning, however, the case of which is somewhat peculiar, and has no doubt appeared to many to support the hypothesis of preservative expulsion of the poison from the system. We refer to those instances (which were far more common in the days of recklessly large *doses* of strychnia than they are now) in which patients have taken a certain quantity of strychnia two or three times daily for many weeks together with impunity, when a not very large increase of the dose has at once produced alarming symptoms. Here, it may be suggested, is a case in which the natural elimination was sufficient for the daily preservation of the organism against the evil effects of a certain quantity of strychnia: but the moment this line was passed, the excess could not be disposed of by the eliminating organs, and therefore acted in an evidently poisonous manner on the system. Such was certainly the impression which the facts originally made upon my own mind; but I have seen reason to change my opinion.

All the recorded instances of this kind were in the early period when toxic doses were given medicinally; they were cases in which the organism was gradually accustomed to the taking of very considerable daily doses, which would certainly have been poisonous to an inexperienced constitution; and it appears to me far more probable that a habit of strychnia-oxidation had been set up in the body, than that the various emunctories had become trained to the elimination of larger and larger quantities of unchanged strychnia. At any rate it cannot be said that a shadow of scientific proof exists for the latter view, either from special experiments with strychnia, or indeed from any evidence that we possess respecting the behaviour of the organism towards any of the narcotic substances in respect of which a "tolerance" is apt to be set up by long use. And this remark brings me naturally to the consideration of another alkaloid, which the organism learns to tolerate far more extensively and permanently than it will tolerate strychnia—viz. morphia.

Of morphia it is now certainly known that a portion does always escape unchanged in the urine. Opinion on this point has fluctuated a good deal; but the experiments of Dragendorff and Kauzmann have put the matter beyond doubt as regards the effect of poisonous doses on dogs and on human beings. These observers found that the elimination in men went on as long as

symptoms of narcotism were present. They also made the interesting discovery that morphia taken by the stomach by dogs is deposited in the liver, and may subsequently pass away in the bile. There is no quantitative proof, however, that the whole of the morphia is disposed of by simple elimination; and strong reasons may be advanced for believing that this is not the case. As regards dogs, it is worth while considering the known fact of the extreme insensitiveness of these animals to opium in any shape—a peculiarity which does not depend on destruction of the poison by the gastric or intestinal juices, for it is equally evident when we inject morphia into the peritoneal cavity. This fact came out so strikingly in experiments which I made when preparing my work on “Stimulants and Narcotics,” that I was led to suppose, at the time, that an extraordinary activity of kidney elimination must be the cause of the immunity of dogs from fatal effects of morphia. It was evident that the poison is taken up into the circulation. A small terrier (black and tan) had two grains of acetate of morphia injected into his peritoneum in aqueous solution; beyond a little drowsiness and slight itching no effects were produced. Three days later ten grains of acetate of morphia were injected: the animal showed no signs of poisoning for three hours, but he then sank gradually into characteristic, but not very profound, opium coma, and remained in this state for seventeen hours, when he somewhat suddenly became profoundly insensible, and suffered from violent and frequent epileptiform convulsions during nearly twenty-four hours, after which he gradually and entirely recovered. It happened that this dog, and one or two others on which I experimented with similar results, passed a good deal of urine; and at the time I was tempted to the provisional conclusion that the comparative immunity of dogs from the fatal action of morphia must be due to a very large discharge of the poison through the kidneys. In 1867, however, I had the opportunity of examining this question afresh. I injected ten grains of morphia into the peritoneum of a dog, and produced symptoms closely resembling those described above; but there was no discharge of urine at all during the twenty-three hours which elapsed before the animal began to get better. As he was then beginning rapidly to recover, I obtained some urine from the bladder, which must have represented the whole secretion since the administration of

the morphia: to my surprise it was by no means strongly impregnated with the poison; indeed there was difficulty in obtaining evidence of the presence of morphia. On the other hand, it seemed certain that the morphia had been wholly absorbed from the peritoneal cavity; for on opening the belly, and turning out the intestines coil by coil, not a trace of powdery or crystalline substance could be seen; moreover, after rinsing the whole intestines (tied and removed) in a small quantity of distilled water, and rinsing out the cavity of the belly with another small quantity of water, no morphia-reactions could be obtained on application of the usual tests to the mixed fluids.

In the next place, a strong argument against the idea that complete elimination of morphia takes place is afforded by the case of confirmed opium-eaters. It is difficult, indeed well-nigh impossible, to believe that, in instances where quantities equivalent to ten or fifteen grains of morphia have been daily swallowed, the whole, or even the greater part of this can have been disposed of by simple elimination: and as such practices have been continued in many recorded cases for years together, we cannot explain the matter by the hypothesis of a temporary storing up of the excess of the poison in the extra-vascular parts of the body. That argument would break down upon a similar *reductio ad absurdum* as Dr. Dupré applied to it when used respecting alcohol. When, on the other hand, we consider the events in acute poisoning of human beings with considerable doses of morphia, we find little reason to suppose that the influence of elimination is sufficiently effective to protect life in any important degree. Although morphia much more frequently fails to kill, when given in large doses, than strychnia, there is this to be remarked—that, if fatal at all, it nearly always kills within a few hours (two to six hours according to Taylor); and it appears exceedingly improbable that the numerous persons who have survived the taking of quantities varying from two to ten grains (not to mention the rarer instances of recovery from thirty, forty, or fifty grains) have really eliminated a sufficient part of this quantity to save life, within so short a time as six hours. This is improbable, in the first place, because the urine, which seems to be practically the only channel of escape,¹ at any rate during the first twenty-four hours, does not appear to carry it off at all rapidly—indeed there is often very little urine secreted

during the first few hours. Again, it is extremely likely that a large proportion of the recoveries from high doses are due to the poison being, from some cause, only very partially absorbed into the blood; we are justified in this opinion by the much more certain and uniform effects of morphia when subcutaneously injected than when given by the mouth. I think I am entitled, by a large and long-continued experience of hypodermic injections of morphia, to say that there are exceedingly few men, *unaccustomed to opiates*, whom a subcutaneous injection of one grain and a half of morphia, applied with thorough skill and effectiveness, would not certainly kill; and that those few persons possess that idiosyncrasy of insensitiveness to opium in all forms which is a well-known though rare phenomenon; for even with such first doses as half a grain, if well injected in a good solution,² I have almost constantly observed strong narcotic effects, except when some of the conditions acknowledged to be especially antagonistic to narcotism (tetanus, very agonising pain, &c.) have been present. And for ordinary purposes one-sixth of a grain will produce a perfectly calculable and sufficient effect on all but a very small minority of patients. Looking, indeed, at the phenomena of hypodermic injection of morphia, I find it entirely impossible to believe that against morphia, once fairly introduced into the circulation, the process of elimination exercises any preservative influence that is of the slightest value in deciding the question of death or recovery.

On the whole, viewing more especially these latter facts and the facts of habitual opium-eating, the following seems greatly the most probable supposition:—That the escape of a portion of morphia by the emunctories is of the same purely physical nature as that which we saw reason to attribute to the escape of arsenic, &c.; and that another and probably not inconsiderable part of the morphia (especially in cases where a “habit of tolerance” has been set up) is disposed of by way of chemical

¹ So far as I am aware, no observer has yet detected morphia in the perspiration. In a case of morphia-poisoning witnessed by myself, the sweat on the face was so copious that three drachms of it were readily collected, but it yielded no traces of morphia, on testing the residue after evaporation (chloroform and crystallisation test).

² It is an ungracious thing to say, but I cannot help feeling sure that a large number of those who use the hypodermic syringe do it with but partial effect, from want of skill or use of an improper solution.

destruction—probably by oxidation, seeing that morphia is a highly oxidisable substance. At any rate we are justified in absolutely rejecting the example of morphia-poisoning as an argument in favour of the tendency of the organism to rid itself of disease by expelling a poison.

Perhaps one of the most decided and at the same time peculiar instances of elimination is afforded by curarin. The researches of Voisin and Lionville have shown that animals poisoned with this substance secrete urine which, when subcutaneously injected into frogs, paralyses them exactly as curara itself would do: and Bidder has shown that the urine of a curarised frog can be employed to curarise a second frog, and that the urine of the second may even be employed to curarise a third. At the same time, it has been observed that curarin does not escape with the bile, and that it does not transude into serous effusions that take place in animals that have been curarised for experimental purposes. It probably escapes (though to a much less extent than in the urine) in the sweat, the saliva, and the tears, all of which secretions were greatly augmented in the few instances of accidental curara-poisoning of human beings that have been scientifically studied. In the instance recorded by Preyer the dose was small, and no symptoms occurred till after five hours: but the sudden augmentation of the secretions just named is said to have been followed by a feeling of relief. The example of curarin, and the even more striking case of that Siberian variety of amanita¹ with which the Tartars intoxicate themselves (and when sobered repeat their debauch by drinking the urine that they have passed during narcotism), must in fairness be allowed to favour the notion, as far as they go, of protective elimination: and did the fate of other alkaloids, especially the more dangerous, really correspond with theirs, something like an argument from analogy might doubtless be built on the fact by those who imagine that zymotic diseases are cured by elimination of the morbid poison. We have seen, however, that in the important instances of strychnia and morphia the evidence points very distinctly the other way. But there is a further important consideration that must not be neglected, and which supplies an

¹ After all, nothing *quantitative* is known about the elimination of either of these poisons.

argument converse to and complementary of those which I have already advanced. If the teleological view of the phenomena of alkaloidal elimination be the true one (and it must be remembered that unless a fairly accurate adaptation of means to end be proved the example is worthless for the support of eliminationist views of the cure of disease), surely we ought to be able to find evidence that, whereas the dangerous poisons are expelled very rapidly and completely, those alkaloids that are harmless, or even exceedingly useful, are either not eliminated at all, or only very partially so. Now apply this rule to the case of quinine. So far from elimination of this substance being specially slow or incomplete, it is quite exceptionally the reverse. The researches of Chau¹ prove *quantitatively* that for the most part, both in health and in the febrile state, very nearly the whole of the quinine is discharged by the kidneys, unchanged, and that this process is completed in about twelve hours after the administration. The only difference between the case of febrile disease and that of health seems to be that in the former the *second* period of six hours, and in the latter the *first* period of six hours, after the administration, is the time during which the largest part of the elimination takes place. These facts are the more remarkable because the well-known researches of Bence Jones and Dupré make it probable that quinine has (to a small extent) a quasi-nutritive function to fulfil in the organism: a quinine-like body being in minute proportion a normal element in the fluids and tissues of the body.

I may probably recur, on some future occasion, to the subject of this paper: at present I shall conclude with the remark that the above examples, fairly chosen from the different varieties of alkaloids, lend no support whatever to the idea that preservative design is the basis of the phenomena of alkaloidal elimination. Here, as before, we are only able to recognise the ordinary physical phenomena of endosmose and exosmose, and mechanical circulation by diffusion, supplemented of course by the action of the heart, which pumps the blood into the ramifications of the arterial and capillary systems.

¹ See *Practitioner*, vol. ii. 1869, p. 60, quoted from *Deutsches Archiv f. Klin. Med.*

ON THE INFLUENCE OF BELLADONNA ON SWEATING.

BY SYDNEY RINGER, M.D.

THE remarkable influence of belladonna applied to the breast in checking the secretion of milk, led the writer to try its influence on sweating. He first employed belladonna in a case of unilateral sweating. A man, forty-five years old, had been troubled for many months with very profuse sweating of the right side of the face and neck, breaking out on the slightest exertion or excitement, or when near a fire, so that the sweat ran down his face and neck in streams, soaking his collar and the band of his shirt, his face being neither red nor congested. The perspiration produced an abundant crop of miliary vesicles, which were strictly limited to one half of his face. The liniment of belladonna applied two or three times a day abated this copious sweating considerably, and reduced it to a little more than the natural amount, and this improvement lasted about six months after the discontinuance of the application, and then the sweating gradually returned, till it became as bad as ever.

The writer has many times checked the sweating of the head and face of young children, often so profuse as to soak their hair and the pillow on which they have been sleeping. Again, belladonna, by means of the ointment or liniment rubbed in two or three times a day, he has several times checked the profuse sweating of the hands, which is sometimes so copious as to run off them in drops, and is especially noticeable at the finger-tips and thumb-balls. Sometimes the good effects are permanent, sometimes the sweating may not return for a considerable time; but occasionally, however, this treatment fails.

The following curious case of unilateral sweating illustrates the beneficial effects of belladonna:—Mrs. P——, aged thirty, married, has all her life sweated freely, but much more on the left than on the right side of her body, the excess being most marked on the head and trunk, although the left arm and leg sweat more than the right. The line of demarcation on the face is sharply defined, equally dividing the head down the centre. The sweating on the left side is very profuse, running down her face and soaking her hair and even the bed-pillow. The sweating on the left side of the trunk is most marked, as low as the breast. Slight exercise, sleeping, or exposure to the heat of the fire or sun, especially the latter, greatly augments the sweating. It is markedly profuse when she is out of health. The sweating is unaccompanied by flushing, and does not excite any rash. She suffers from great coldness of the feet, but the right foot is decidedly the colder. She never feels the left hand warmer than the right. The left side of her tongue is always more coated than the right, and has been so as long as she can recollect. There are no bad teeth or other causes in the mouth to account for this curious fact. She is rather deaf on the left side, and when young was very deaf of both ears, but much worse on the left side. Her pupils are equal, and her sight good in both eyes. She is very hysterical, and suffers often from globus hystericus, and from a sensation of heat and weight on the top of her head and palpitation on exertion or excitement. Her urine varies greatly in quantity, sometimes being scanty, at other times very abundant. Her bowels are generally confined, and she is troubled with a cankerous taste in the morning. Her menses are very scanty and irregular, intermitting sometimes for six months. She does not blush more one side of the face than the other; and when excited her ears become both equally very red. The skin of the face presents the same aspect on each side. The radial pulses appear to beat in all respects equally. Five months ago she was confined, and since then all her troubles have much increased. The left breast yields much less milk than the right, this being full and distended, while the left is flat and empty. Shortly before her visit to the hospital she was seized with neuralgia of the auriculo-temporal branch of the inferior division of the fifth nerve on the left side, the paroxysm being accom-

panied by salivation of the left side of the mouth ; but previous to this neuralgic attack the secretion of saliva was no greater on one side of the mouth than on the other. During an attack of pain the left side of the face sweated greatly.

She has lost two children—one from measles, the other from hemiplegia. Deafness is a family complaint ; no other member of her family suffer from nervous disease, and none are affected with unilateral sweating. At fourteen years of age she suffered for a year with twitchings of the right side, the arm being especially affected, but this does not appear to have been true chorea : and still on excitement there is a disposition to involuntary movements of the right arm.

The application of a weak belladonna ointment to the left side of the face for five to ten minutes, three times daily, greatly reduced the sweating and equalised that of the two sides. It moreover cured the neuralgia.

The author has met with cases of profuse local sweating over the loins, covering a surface rather larger than the hand, and exciting a copious eruption of eczema. Here the belladonna liniment checked the sweating, and the eczema at once disappeared.

In some cases of sweating the belladonna no doubt fails. Thus in one inveterate instance, so far from affording relief, it increased the sufferings and sweating in the case of a man who for twenty-two years had been affected with an occasional eruption of the hands and feet, looking at times like eczema, but at other times putting on the appearance of lichen. In cold weather he is pretty well, unless he takes exercise or sits in a hot room ; but in March, as the weather begins to get warm, the symptoms set in. His hands and feet swell and feel so tight as if they must burst. At the same time he sweats a good deal, but not while lying down ; but directly he rises, or even sits, the sweating begins. The sweating is much more abundant in his hands and feet, especially at the finger-tips and the thumb-balls ; and at the tips of the ring and little fingers of the left hand it is especially marked. The sweat runs down in drops from the hands, and when he wipes the finger-tips he can see the sweat oozing from the pores of the skin. The outer part of his hands corresponding to the fifth metacarpal bone is also the seat of great sweating. At the tips of his fingers and the thumb-balls he suffers from severe

pricking pain, which he likens to little insects biting their way out. He feels hot all over, and calls it heat in the blood. The attacks are accompanied by a good deal of itching over his back. The skin of the hands about the nails becomes hard, cracks, and bleeds. A little rash similar to that described appears in the clefts of the fingers and over the back of the hand between the thumb and fore-finger. In this patient's case, as has been stated, belladonna ointment applied to the hands greatly aggravated all his troubles and increased the sweating.

In order to test still further the effects of belladonna on sweating, many experiments of the following kind were performed by Mr. C. A. Nankivell on several occasions:—A patient in University College Hospital, after undergoing a sweating in the hot-air bath, was rubbed with belladonna ointment on one side of the face for ten minutes, three times a day, for two or three days; then the bath was repeated of the same temperature and duration, when it was observed that the sweating both during and subsequent to the bath was very greatly lessened, and that the effect was general, although the ointment was applied only to one side of the face. On some occasions the ointment was rubbed into the chest, but then the effects were much less marked than when applied to the face, possibly because less of the ointment was absorbed.

As the local application checked sweating over the whole body, it was concluded that it acted by its absorption, and this led to the internal administration of belladonna, but its repressing effect was apparently decidedly less than when locally applied, possibly because less of the drug was given by the mouth than was absorbed by the skin. Still, no doubt the internal administration of belladonna does sometimes effectually control sweating, as the author has often witnessed in the case of weakly children perspiring profusely on exertion or whilst sleeping; and in the following curious case of a middle-aged man who, after much mental worry, suffered from excessive sweating of both cheeks while eating, especially hot meat or vinegar, the sweating ceased immediately after the meal. This man passed at times a profuse quantity of pale urine. Ten drops of tincture of belladonna taken three times a day completely checked the sweating.

Since writing the foregoing, the author has made some further experiments with very striking results. A middle-aged man, a sufferer for several years from unilateral sweating of the right side of the face and neck, applied for relief from a pain in his side. He was found to be sweating very profusely from the right side of his face and neck, from exertion and the great heat of the weather. To relieve his pain a twelfth of a grain of morphia was subcutaneously injected, which appeared to increase the sweating, although it was difficult to be sure of this: while still sweating profusely, so that on wiping his face the sweat could be seen rapidly oozing from the skin, we injected $\frac{1}{100}$ of a grain of atropia under the skin of his arm, and in about a minute the sweating entirely ceased, and his face remained quite dry, till his dismissal about three-quarters of an hour after the experiment.

To a middle-aged woman suffering from acute rheumatism, a hot-air bath was administered, followed by cold sponging. This treatment caused her to sweat so freely that for several hours after the perspiration continued to pour down her face, soaking her clothes and the bed-linen. While in this state $\frac{1}{100}$ of a grain of atropia was subcutaneously injected into her arm, and in about a minute the perspiration ceased, and for two hours her skin continued dry and she felt much cooler, but in the evening rather free perspiration returned. We next gave a young man a Turkish bath, and Mr. Johnson, the resident assistant of wards, who has helped me in these observations, joined him in the hot chamber. Both sweated freely, and then each was injected with $\frac{1}{100}$ of a grain of atropia, and in a little more than a minute the skin became dry, and the perspiration did not return after the application of the cold douche, nor afterwards. Mr. Johnson remarked that so dry did his skin seem, that he felt he should never sweat again. They both suffered from much dryness of the mouth, but their pupils were not dilated. We next placed a boy in the hot-air bath, the temperature rising to 180° Fahr., and when sweating freely we injected $\frac{1}{120}$ of a grain of atropia, and almost immediately the sweating ceased and did not return.

CASES ILLUSTRATING THE ACTION OF CONTINUOUS ELECTRIC CURRENTS ON SPASMS OF THE BLADDER, URETHRA, AND URETERS.

BY DR. RELIQUET (OF PARIS).

I.—IN the month of March, 1869, I had a patient suffering from stone in the bladder, with contraction of the bladder over the stone. Micturition took place almost every instant, and it was quite impossible to inject more than ten grammes of tepid water into the bladder.

I resolved to make use of electricity, and, with my friend Dr. Onimus, applied continuous electric currents in the following way. A gum-elastic sound, furnished with a very flexible stilet, was introduced into the bladder; the usual ten grammes of water were injected, and the sound closed. The positive electrode was then applied to the stilet, and the negative (a large moist plate) to the hypogastrium. At the beginning of the current there was a little pain, which soon disappeared, and then complete composure ensued. After four minutes the currents were stopped; then tepid water was injected into the bladder, and as much as 150 grammes were introduced without the patient feeling the desire to make water. I availed myself of the opportunity to examine the stone, which I found lodged in the vesical fundus, where it was impossible to lay hold of it with the lithotrite. The painful spasms having returned under the provocation of these exploring manœuvres, I resumed the continuous currents, which immediately brought back the calm which they had at first produced.

This brusque dilatation of the bladder occasioned the loosening from the vesical parietes of crystalline incrustations, which the patient passed during the forty-eight hours which followed this first application of electricity.

On the following days I renewed the application of electricity, and each time I invariably obtained the same temporary sedation of the vesical contraction. However, after the first micturition which followed, the spasmodic condition of the bladder again returned.

In this case the problem so eagerly sought to be obtained by the most various means, and especially by the local action of carbonic acid—namely, *to dilate the contracted bladder*—seems to have been solved.

Consecutive applications of continuous electric currents in various cases of painful spasms of the urinary passages, have determined in a definitive manner the practical importance of this novel remedy.

II.—In the month of October, 1871, I again applied continuous electric currents to act against the contraction of the bladder over a calculus. The results which I obtained were absolutely identical with those of my first case. Only thirty grammes of tepid water could be tolerated by the bladder: by means of the current I was enabled to inject 130 grammes without bringing on any desire to pass water. But, as in the former instance, the desire to pass urine became more and more frequent after the first micturitions which followed, and the condition of contraction did not fail to return: the direct cause of the phenomenon, namely the stone in the bladder, being still present.

III.—In the month of January, 1870, I attended a young man of twenty-one, who came to consult me, he said, for catarrh of the bladder. And, indeed, the urine contained much muco-pus. Moreover, the desire to pass water was most frequent. The jerking jet of water at the commencement of micturition did not persist; at the end the urine passed in drops. There was constant pain in the anus, and pricking sensations in the neighbourhood of the glans. Pain was especially intense after each micturition. On examining the bladder, I found that it did not empty itself at each micturition, though it could not bear more than 100 grammes of tepid water. Yet I could find no calculus. After this investigation, which had been carried

on with the slightly curved instruments, the patient was two days without feeling any pain in the anus or penis. They again appeared, however, after that time, and renewed depression of the lower lip of the neck of the bladder produced no effect.

I then applied the continuous electric currents. As in this case it was contraction of the urethra which dominated, the bladder not emptying itself entirely, I applied the negative electrode in the bladder, and the positive electrode over the hypogastrium. After having thus made several intermittences, I allowed the continuous current to go on. Then, putting the positive electrode on the perinæum, I obtained a current crossing the deep region of the urethra, and going from the perinæum to the bladder. After five such applications the desire to make water was only felt three times in twelve hours; the bladder could receive 220 grammes of tepid water without there being any wish to urinate, and furthermore could effectually empty itself. Pain had completely disappeared; motions, which had been difficult, had now become easy, natural, and daily. The urine contained scarcely any muco-pus.

Walking, and especially dampness of weather, however, had the effect of awakening the condition of pain. But new applications of the continuous electric currents arrested the painful spasms, and again caused disorders of micturition to disappear.

At the end of February, the patient, on returning one night from the theatre, was seized with a violent nephritic colic of the right side. At the same time the urine was very thick, and micturition became quite frequent. I immediately had recourse to the continuous currents, applying the positive electrode to the back in the situation of the right kidney, and the negative electrode over the perinæum. I made a long *séance* of a quarter of an hour. After this the night was passed in complete calm, and the patient passed in making water, almost without being aware of it, a tolerably large quantity of gravel. On the following days I continued the employment of the electric currents, still following the direction from the left kidney to the bladder. Complete recovery became more and more evident. Ten days after the escape of the gravel, I carefully examined the bladder with the exploring lithotrite, but discovered nothing. The bladder could then tolerate 220 grammes of tepid water,

and micturition took place regularly every three or four hours. The urine was quite normal.

It seems obvious from this fact that (1) continuous electric currents first cause the arrest of spasmodic phenomena of the urethra and bladder excited by renal gravel and by the alteration of urine which results therefrom; and (2), a most important fact, that the action of electricity has been most effective against the spasms of the right ureter, since, by occasioning the cessation of the spasms, it excited the evacuation of the renal gravel.

In the following case, which I have briefly summed up, it will be seen that, in a very irritable subject, electricity, in causing the disappearance of the spasms of the urethra, also caused the permanent disappearance of disorders of micturition.

IV.—In the month of August, 1871, Mr. H——, aged 45, of an extensively developed neuropathic temperament, came to consult me. He had just returned from Contrexeville, whither he had resorted with the hope of getting rid of various urinary troubles from which he had been suffering for several months.

On examining him I remarked the presence of the following symptoms:—Frequent desire to urinate: nine or ten micturitions in the night; not quite so many during the day. There is a thick sediment of muco-pus in the water, but no crystals or earthy matter. Moreover, the urine has no smell of ammonia. Each micturition is accompanied with an intense burning sensation in the urethra, and followed by a painful sensation going from the anus to the meatus urinarius. There is habitual constipation.

This constipation I immediately attacked by prescribing a glassful of Pullna water every two or three days, and the use of fresh vegetables.

On August 29th, I introduced easily into the urethra a gum-elastic sound of five millimetres in diameter. Its passage through the deep region of the urethra excited intense pain. By means of the sound I drew from the bladder more than 200 grammes of urine, a much greater quantity than that afforded by each micturition, and which proved that the bladder did not entirely empty itself at each micturition. After having

injected tepid water into the bladder, I attempted to introduce the exploring curved catheter; but as soon as the beak of the instrument attained the membranous portion, intense pain ensued, and consequently contraction of the urethra, which completely put a stop to any further introduction. I therefore contented myself with passing a metallic sound, with a less marked curve, by the aid of which I explore bladders in which I have discovered nothing.

After this exploration the pain in making water became much more intense, and micturition still more frequent.

The following day, August 30th, I applied continuous currents, putting the positive electrode in the bladder, by means of a curved sound furnished with a brass wire (moving freely within the sound), and the negative electrode over the perinaeum.

After five minutes' employment of a perfectly continuous current, I applied the negative electrode to the hypogastrium, where I made various interruptions of currents. In this way I made four *séances* of electricity, one every day; and then four others, one every two days; making in all eight sittings.

From the very first sitting, the pain in making water became much less intense. As pain diminished, so the quantity of urine afforded by each micturition became greater, and the desire to make water more and more rare. After the sixth *séance* there were only two micturitions in the night, and during the day the patient remained more than three hours without any wish to urinate.

The water gradually became normal as the disorders of micturition disappeared. I may just state that the patient was taking an infusion of pine-buds.

Two months later, at the end of October, I saw Mr. H—, and he was still doing remarkably well.

As is shown by this case, the spasms of the urethra cease, and the complete contractility of the bladder disappears, under the influence of continuous electric currents properly applied.

In cases of spasms of the deeper portion of the urethra due to chronic urethritis of this part of the canal, I have always obtained the abatement of the spasms, and consequently a diminution of functional troubles. The patients make water less often; the water does not run out by jerks, and the comparatively considerable effort made to eject the urine, which is so slow

in issuing, decreases much, and often is reduced to the normal effort which attends the beginning of micturition. But in these cases, when we do not succeed in dispelling suppuration of this deep portion of the urethra, all the spasmodic phenomena return as soon as the applications of electricity are suspended.

This abatement of functional troubles procured by the continuous currents is a most favourable condition, which increases the efficacy and play of the therapeutical means employed in chronic urethritis.

When, whatever may be the cause of functional troubles of micturition, the patient is an irritable subject, there is need at each *séance* to begin by electrising the lower part of the spinal cord with a descending current: the reflex power is thus diminished. It is by this means that Dr. Onimus has been enabled to cure nocturnal incontinence of water in several children; but with these patients it is important not to act directly upon the genital parts.

We are now in possession of a means of action against painful spasmodic phenomena of the urethra, of the bladder, and of the ureters, and, what is very important, of a remedy which produces no inconvenience in the general organism, as have done all the medicinal substances employed until now, as, for instance, opium, belladonna, and bromide of potassium. Moreover, as all the facts of my experience have shown me, the action of continuous currents is immediate. We are able to calm instantaneously pain produced by the contraction of the bladder over a calculus, and that until the bladder again becomes irritated over the stone. And, during this time of temporary dilatation of the bladder, of vesical tolerance, we are enabled to act on the stone. Whence arises a most important indication amidst many others, namely, the possible advisability of applying continuous electric currents immediately before a *séance* of lithotripsy.

Reviews.

Ueber die Ernährung der kranken vom Mastdarm aus, Nach physiologischen Experimenten und klinischen Beobachtungen.
Von Dr. W. O. LEUBE. Leipzig: Vogel, pp. 54.

THIS pamphlet describes an interesting and very practical series of researches, directed to the object of discovering the real nutritive value of food-enemata, and the best means of preserving life by their employment when gastric digestion is impossible.

Dr. Leube arrived at the conclusion that the addition of pancreas-extract, or (preferably) pancreas in substance, to meat, is highly important if we desire to make an effectively nutritive material. The following is the description of the nutritive enema with which he obtained excellent results:—

Beef is prepared by shaving it into thin slices, and then chopping it up finely: of this material from five to ten ounces are mixed with a third part their weight of pancreas, freed from fat and finely minced. (The pancreas of pig or ox, quite fresh, is to be used: this is of little value to the butchers, and can be had very cheap. Care must be taken not to select those *transparent* specimens of the gland which, as Schiff and Kühne have shown, have no digestive power.) The mixture of beef and pancreas is to be put into a mortar with about five ounces of lukewarm water, and reduced to the consistence of a thick soup with a pestle or a large spoon. The addition of the water is absolutely necessary to make the injection fluid enough to pass freely through the syringe: and the water must not be cold, or it will irritate the rectum and produce expulsive action. If it be thought advisable to add fatty matter to the enema, this is added in the proportion of one-sixth of the meat: and a thorough emulsion is made.

An enema so prepared is not very readily injected with common instruments, and Dr. Leube therefore devised a special apparatus by which considerable force can be exerted without doing any damage, but which it is not very easy to describe

without the aid of the engraving which he gives. It is in reality, however, very simple.

The results obtained by working in this manner seem to have been highly satisfactory. First of all, it is reassuring to find that these enemata are not in the least liable to excite expulsive action; on the contrary, they penetrate a good distance beyond the sigmoid flexure, and remain there for perhaps twenty-four hours, being thoroughly digested and absorbed: and such motions as the patient does pass, are nearly or quite indistinguishable from ordinary fæces. The following *résumé* is worth translating entire.

1. The injection of a mixture of finely divided meat and pancreas into the rectum almost never produces diarrhœa; on the contrary, the material lies from twelve to thirty-six hours in the large intestine without producing evacuation. The latter is easily provoked, however, if fat be added to the enema, if the fat be more than one-sixth the weight of the meat; otherwise fat may be given with impunity.

2. The administration of pancreatic enemata should always be preceded by the use of a water-clyster: for the large intestine must be cleared of fæcal masses before it is fit for the digestion and absorption of the enemata. Even if there has just been an action of the bowels the water-clyster should still be used.

3. The first nutritive clysters which the patient gets are often, apparently, not digested: we must not therefore draw unfavourable conclusions from the non-effectiveness of the procedure at first.

4. If, after several nutritive injections have been well borne, one comes away sooner than usual, it is better, in order to favour the intestine, to give a day's rest from injections.

5. The above-described materials for enemata are to be recommended above others, for nutritive purposes, because of their effectiveness and facility of application, and also because their composition is specially adapted to the ordinary digestive processes that go on in the large intestine.

6. The patient suffers no uneasiness after the injection of the pancreatic enemata: not only is there almost never any pain or oppression in the abdomen, but there is often, on the contrary, a sense of comfort, a loss of the feeling of abdominal emptiness, and sometimes the complete cessation of hunger.

7. The injections always produce at least a temporary increase in the fulness of the pulse, an improvement of the general condition, and a relief to the anxiety of the patient.

The Graft Theory of Disease; being an application of Mr. Darwin's Hypothesis of Pangenesis to the Explanation of the Phenomena of the Zymotic Diseases. By JAMES ROSS, M.D., Waterfoot, near Manchester. 8vo. pp. 292. London: Churchill.

[FIRST NOTICE.]

THE work before us is one of a class which seldom comes under the notice of medical readers. We are not at this moment pronouncing any judgment on Dr. Ross's volume, but we say at once that it is a specimen of a kind of work the rarity of which in medical literature is a reproach to our profession. It is an attempt to carry out in a strictly philosophical manner an inquiry into the relations between the phenomena of zymotic diseases and those of biological reproduction. Dr. Ross's method of working is one that is rarely employed by writers on medical subjects, and we do not doubt there will be plenty of hasty critics to exclaim at his book as "mere theory!" and so forth. It is strange how many persons there are who believe, in a muddle-headed kind of way, that what they call the inductive method has superseded all need for theory in scientific matters, and are quite unaware—this is the especial foible of the self-called "practical" men—that the necessity for some theoretical basis to our scientific belief is so imperious that if not satisfied in the legitimate way it will inevitably lead those who neglect it to the acceptance of unconscious hypotheses as if they were proven facts. Since everyone who undertakes to write on pathology must theorise whether he knows it or not, it is as well that, like Dr. Ross, he should be fully aware of what he is about.

The question which our author raises is that of the nature of the contagia of zymotic diseases. He briefly but clearly states the position reached, so far, by modern researches: and starts on his fresh inquiry from the assumption (which few will dispute) that Chauveau and Burdon-Sanderson and Beale have decidedly proved the contagia of vaccinia, small-pox, cattle plague, and some other zymotic diseases to consist of microscopic bodies, some $\frac{1}{20000}$ inch in diameter, which closely resemble bacteria in many important particulars, although there is no proof of their identity with the latter: and that there is strong ground for the inference that *all* zymotic diseases have contagia of the same general character as that just described. Moreover, it seems plain that these contagium-particles are living. The special inquiry of Dr. Ross begins from this point: he attacks the question—whether these living particles have each of them the life of an independent organism, or are merely cast-off particles

of one organism, capable of retaining vital properties and flourishing in the soil of another organism.

The former of these two hypotheses is, of course, what is known, in general terms, as the "germ-theory" of zymotic disease: and accordingly the first business of our author is to trace, as carefully as possible, the more important shades of opinion among the advocates of that theory. He succeeds, we think, in showing that at any rate this hypothesis is not the only one that can explain the facts of contagion, nor does it explain them in such a manner as to leave no serious difficulties: and then he undertakes to propound another hypothesis which will explain them better. He calls this the "Graft Theory:" it is an application of Mr. Darwin's hypothesis of pangenesis to the phenomena of zymotic contagion. Speaking in general terms, this hypothesis supposes that the contagious particles are not independent beings, but merely portions cast off from another organism, which particles, still living, implant themselves in the body, and set up changes in it analogous to those produced in the stock of a tree when a graft from another tree has been inserted into it.

We think that to anyone who has reflected on the subject, quite apart from any consideration of the theory of pangenesis, the phenomena of grafting are remarkably suggestive of the effects of the contagia upon the human organism. The strange permanence of the effects left after subsidence of the nutritive disturbance excited in the stock by the first presence of the graft reminds one of the singular transformation of the human organism, which is indicated by its insensitiveness, either permanently or for long periods, to the contagion of a zymotic disease from which it has already suffered: and it is needless to say that a similar permanence and profundity of effect upon the organism is observed in the analogous phenomena of ordinary generation in the domesticated mammalia. The bitch that has once been impregnated by a particular dog is apt to transmit particular features of that dog to subsequent litters of pups produced by other fathers. She is, in fact, a different animal after her impregnation by the first dog, from what she was before it; the commotion excited in her system by the fertilisation of those first germ-cells by no means ended with the maturation and expulsion of a certain number of fetuses; on the contrary, she distinctly retains in her some elements of the dog's organisation. It can hardly be doubted that, in a vague way, these facts must have suggested to many minds the idea that the contagia of zymotic diseases may be something much more than mere fermentative agents, exciting a storm of commotion, and then leaving the organism empty, swept, and garnished; that the process of "cure" in fact is, as Sir W. Gull and Dr. Handfield

Jones some years ago hinted, a readjustment of the equilibrium of the organism to the presence of new elements that have become incorporated with it. And along with these general ideas, which have been, in fact, common property for some time past, there may have existed in some minds, as certainly there existed in our own, a suspicion that the hypothesis of pangenesis might be found to explain a great many pathological phenomena more simply than the current theories.

It is one thing, however, to entertain a hazy notion that something might be made of an idea, and quite another to work the application of that idea out, with clear and logical method, as Dr. Ross has done. It had been our intention to review his book in full in the present number; but so difficult is the subject, and so great would be the injustice of hurried criticism upon a work which teems with original thought, that we must reserve our fuller notice till next month. Meantime, we have been anxious to direct the attention of the profession to the book without delay, for it would be a pity if a work of such high quality did not at once receive the compliment of careful consideration by competent men. And we venture to remark, in anticipation of our fuller criticism, that the book must not be looked into casually, but read in proper sequence from beginning to end. There are some speculations, for instance, in one of the later chapters, which, if taken alone, might lead to an unfavourable judgment on the whole work: and indeed we are not sure that they can be fully justified, even when considered in their proper place and order. The fact is, however, that they belong to a subsidiary or collateral argument, which may to some extent be called gratuitous, though it will be found to contain matters of great interest. Indeed, unless the critical faculty has entirely failed us, we may say that to those who are looking forward with hope to an English reformation of pathology, which will not be without a powerful influence on the progress of therapeutics, the work of Dr. Ross, whatever its imperfections, will be hailed as an *avant garde* that heralds important events.

Notes on England. By H. TAINE, D.C.L. Oxon., &c. Translated, with an Introductory Chapter, by W. F. RAE. London: Strahan.

ALTHOUGH we have no doubt that readers of this journal are staring with surprise at our seeming freak in noticing such a book as the above in the pages of a therapeutical journal, we hope to make them retract the hasty opinion, which they are certainly entertaining, that our brain has been turned by the

hot weather. So far from our conduct being without excuse, there is quite an *embarras de richesses* in the materials for our defence. We might very well, in the first place, take the high tone of defiance: we might say that an unfortunate editor has just as much right, once in a way, to forsake the dry and sapless materials which science offers as his ordinary mental food, for a succulent meal on the rich vegetation of M. Taine's wit and humour, as King Nebuchadnezzar had to neglect the affairs of his kingdom during that prolonged graminivorous bout that we wot of. We might add, as a pendant to this analogy, that as the great king was decidedly the better for his green diet, we hope for a like improvement in our own mental and spiritual state. But in truth there are less selfish reasons for our choice: for the book before us is precisely of that kind for which the busy practitioner of medicine would do well, every now and then, to neglect all the studies which belong to the round of his daily work. What more wholesome relief from the dull details of attendance upon the sick humours of individual English men and women than to see how English folk in general—their physique and their way of life, no less than their mental qualities—have impressed the mind of one of the acutest foreign observers who ever came among us? There is food for infinite reflection in many a page of this book, light and sparkling as its surface qualities may seem. There are thoughts on the effects, on our race, of the competitive strife of modern society, which doctors would do well to ponder again and again: and there are questions raised as to the true outcome of our English plenitude of muscular and nervous energy, and of the lavishness with which we expend it, as to which medical men will be increasingly compelled to meditate and to form practical opinions. There are studies of our social vices, which are grave, and true, and most suggestive. Above all, there is something which should be especially admirable to a scientific profession, in the *detached* quality (to use a critical slang-word) of M. Taine's views of social and physical life in England. For a Frenchman, this is little short of the miraculous. Let us add that Mr. Rae, the translator, has done his work in a way that very few translators do: he gives a racy (though not finically purist) translation, and he introduces the work by an explanatory chapter of his own, of which it is a high but true compliment to say that it is not dishonoured by comparison with the production of M. Taine himself.

Clinic of the Month.

Treatment of Hernia Cases after Operation.—In the course of his clinical lectures on strangulated hernia, Sir James Paget makes the following observations on the treatment that should be pursued :—“The present general rule of practice after operation, in cases likely to go on well, is to do what is called nothing; to wait till some reason for interference is manifest, and, while waiting, to take care that the patient shall have fit bedding, fit air, fit food, quietude, and good nursing. All these prime conditions of health are called ‘nothing.’ The contrasted something would be bleeding, active purging, or other restless interference with the natural course of recovery, such as was in vogue in the earlier years of my case taking, and such as had not quite ceased twenty-five years ago, when I became assistant-surgeon to the hospital. In this contrast you will see only an illustration of the great change of opinion respecting treatment which may be observed in a wide range of medical practice, as in cases of fever, acute rheumatism, pneumonia, and all acute inflammations—a change shown, not by substituting one remedy for another, but by letting many diseases and the effects of many injuries take their natural course, in the confidence that they will come to a natural good end, and that we have no medicines potent to alleviate or cure them. But let me say that while I have no doubt that the present general plans of treatment are better than the past, I yet do not believe that the past plans of treatment were so mischievous as some have told of them. I have no recollection of serious harm being often done by bleeding in the many cases of illness, whether slight or severe, in which, during my apprenticeship, I practised it. In a few cases I think it was mischievous, but in the great majority it was harmless. In many, it gave such relief from pain or other distress as naturally strengthened the belief that it did real good, and in a few cases I do not doubt that it was beneficial. Still, in cases of strangulated hernia, whether before or after operation, I think you need never bleed a patient. I do not believe that bleeding ever saved the life of a hernia patient, which but for the bleeding would have been lost. And as for purgatives, though I believe they were often mischievous, and more often unnecessary, yet I do not doubt that they were, and still may be sometimes,

very useful, and I wish I could tell you more exactly than I can the class of cases in which they should be used. At present I know only one—the class, namely, in which it is clear that the strangulation has occurred while the bowels are overfilled, and in which the strangulation is acute and quickly relieved.

“Now for general rules of treatment after operations for strangulated hernia. Bear in mind the complicated cases with which you may have to deal. In each case there are, or may be, these constituents: the intestine damaged by displacement, and by being forcibly replaced; the operation-wound; the effects of chloroform; the intestinal disorders which, at least in many cases, preceded the strangulation, and may continue after the operation; the effects of aperients and other medicines given for this previous disorder, or for strangulation; the inflammation, or worse than inflammation, of the sac and its contents, which does not subside immediately after even a successful operation.

“When a case has been timely operated on, all these things may amount to nothing worse than may be left to the course of spontaneous recovery, and a case that goes on well requires that ‘nothing’ in the way of treatment of which I spoke just now; but when anything goes amiss, you must have in mind all the things I have enumerated, in your endeavour to interpret the signs of wrong and to amend it.” (*British Medical Journal*, July 6, 1872.)

Purgative Properties of Sulphovinate of Soda.—In a paper read before the *Société de Pharmacie de Paris* in March last, M. Limouzin observes that the Sulphovinate of Soda has the cool taste peculiar to all the salts of soda; it is nearly free from bitterness, and has a sweet after-taste that makes it more easily tolerated than the sulphate, while as a laxative it appears to be about three times more powerful than that salt, and much more prompt. It is given at the “Charité” to adults in 20- to 25-grain doses, and by Dr. Blache to children in 10- to 15-grain doses, sweetened with syrup. Among the advantages claimed for it over other saline purgatives are, that its administration is not followed by constipation, and that there is not the danger of the formation of vesical calculi that sometimes attends the use of salts of magnesia. Sulphovinate of soda may be administered alone dissolved in *cau sucrée*, or in a draught sweetened with a flavoured syrup, or in solution in water charged with carbonic acid. This last forms a beverage much more agreeable in taste than the citrate of magnesia, and has the advantage of not undergoing alteration for a long time. (*Pharmaceutical Journal and Transactions*, June 8, 1872.)

Action of Sulphate of Quinine upon the Temperature in Pulmonary Phthisis.—In a case under Dr. Ogle’s care at

St. George's, of a young woman suffering from dyspepsia, pain across the back, great debility, and mental despondency, in whom phthisis subsequently developed itself, and in whom also careful thermometric observations were taken, the following points were noticed:—(1) That the temperature was remarkably high and uniform for a case of active phthisis, the temperature during observation, prior to the exhibition of large doses of quinine, being never lower than 100° Fahr., and occasionally 101° Fahr. (2) The promptitude with which, on the exhibition of 10-grain doses of quinine, the temperature sank to 99° , this range being maintained night and morning. (3) The same degree of temperature only being attained when the quinine was increased from 10- to 15-grain doses, except on one occasion, when it fell to 98° . (4) The return of the high range of temperature on the disuse of the quinine, and even of a higher degree, as on three occasions it reached respectively $100^{\circ}2$, $100^{\circ}3$, and $100^{\circ}4$. (5) The subsequent reduction of temperature to 99° on the quinine being given in 20-grain doses night and morning, with only one or two exceptions, when it attained 100° , or a little more. During these fluctuations no proportionate modifications of the respiration of the pulse or of the heart's beat were found to exist. (*Lancet*, July 6, 1872.)

Puerperal Disease treated with Turpentine.—Dr. Copeman records ten cases in which, for various conditions supervening upon delivery, turpentine proved extremely useful. The following is one of these cases. Mrs. —, aged 40, multipara, confined on October 29th. The labour was natural, but the placenta was adherent, requiring extraction by piecemeal, and this was attended with considerable loss of blood. On the 31st she had a severe rigor, followed by rapid pulse, abdominal pain, and scanty, offensive lochia. The uterus was large and tender, and sensorium disturbed. She had another rigor on the following day, and when Dr. Copeman saw her on November 2nd, her pulse was nearly 150; and all her symptoms indicated a severe attack of puerperal fever. She was immediately treated with turpentine internally, and it was also applied externally with hot flannels to the abdomen, and a turpentine enema was administered. The vagina was well washed out with warm water every now and then, and some of Condyl's solution sprinkled about the bed, and applied to the ostium vaginae on a napkin, in consequence of the excessive fetor of the discharge. For a few days she was in a very precarious state, causing great anxiety to all, but then a piece or two of putrid chorion were expelled, and the discharge was less offensive afterwards. On the 6th the pulse came down to 100. She had continued the turpentine in half-drachm doses night and day,

every four or six hours, and such was the relief it afforded her that she often wished to take it before it was due. By the 10th she was so much better that Dr. Copeman's attendance was no longer required, and on December 8th she came down stairs to her sofa. Her recovery was complete. She has had one if not two children since, and is now in perfect health. (*Med. Times and Gazette*, July 6, 1872.)

The Treatment of Ingrowing Toe-nail.—Mr. Stillwell, of Epsom, states that forty years ago, when he was an assistant, a young farmer one day came to the surgery and was operated on for an ingrowing toe-nail. The poor fellow suffered so severely that Mr. Stillwell determined that he would never perform the same operation. In the course of many years' active practice he has had numerous similar cases under his care, and his invariable mode of proceeding has been to find the edge of the nail with a probe, and then remove the whole of the granulations and hypertrophied cellular tissue on both sides if requisite. In no case has he been disappointed, or ever had to treat the patient for a return of the complaint. (*British Med. Journal*, July 13, 1872.)

Ectrotic Treatment of Small-pox and Scarlet Fever.—Dr. Alex. Watson records several cases of these diseases in which the external application of carbolic acid proved of signal service. In one case of small-pox which he saw at the period of the appearance of the papulæ, he ordered enema, and the patient, a girl of eleven, to be sponged all over with tepid carbolic acid soapsuds. On the following day there was every appearance that it was about to be a severe case of confluent small-pox. She was directed to be sponged again with warm carbonic acid soapsuds, and immediately after the whole body to be painted over with the carbolic acid glycerine of the British Pharmacopœia. Some smarting was experienced on the parts that had been scratched, but otherwise only a general glow of warmth. Five grains of Dover's powder were given to allay irritability, after which the patient slept calmly for several hours, and in the evening she was sponged again. No vesicles formed, and in a few days she was convalescent. The atmosphere of the bedroom was in this and other cases saturated with the vapour of carbolic acid by means of sheets hung up in place of doors and windows, and constantly wetted with the solution of the acid. (*Lancet*, July 13, 1872.)

The Influence of Ergot of Rye in Epilepsy.—Dr. Yeats records a case of epilepsy and mania, in which great benefit was experienced from the use of ergot. The patient had been

subject to epileptic and maniacal attacks for twenty years, the former varying in the interval of their return from three to six months, and being succeeded by a violent maniacal attack. He was thin and pale, took his food regularly, and slept well, except for a few nights following each fit. When excited, his pulse was about 110, and the temperature 99.5° . At first bromide of ammonium, in 20-grain doses, three times a day, was administered, but without effect, except that his circulation became much enfeebled, and his feet unusually cold and clammy. On the next attack drachm doses of the liquid extract were administered three times a day: after the third dose had been given a visible alteration took place; instead of being violent and turbulent, he became quiet and temperate in his actions, and slept well, which was quite unusual for him so soon after a fit. On the following day he was still calm and quiet, his pulse having fallen from 108 to 88 per minute, his temperature from 99° to 98° . The ergot was then administered in half-drachm doses, twice daily; after two days the quantity given was reduced to 40 minims in the twenty-four hours, and entirely discontinued in two days more. His appetite kept good all the time, and the secretions were natural. The same results were obtained on several subsequent occasions when symptoms of excitement occurred. On the whole his condition is much improved. (*Medical Times and Gazette*, July 13, 1872.)

Treatment of Erythema Nodosum.—Dr. Purdon, of Belfast, considers that erythema nodosum should properly be classified with the hæmorrhages, since it is ushered in by febrile symptoms, neuralgic pains in the limbs, and the protuberances most frequently seen parallel to the tibia are due to the hæmorrhage, the centre of the oval node-like patch being filled with blood. The lower extremities are not always the seat of erythema nodosum, the arms, for instance, being occasionally attacked. He thinks that vasomotor nerve derangement is an important factor in causing this disease, which usually appears in delicate, pale, thin, and anæmic young girls, often accompanied by chlorosis. In regard to treatment, after approving Dr. Spender's method,—which consists in prescribing sulphate of magnesia with sulphate of iron, and dilute sulphuric acid with sea-bathing, shampooing, and a Domette flannel bandage to increase the temperature,—Dr. Purdon adds that he finds no difficulty in “curing” erythema nodosum by prescribing the compound iron mixture and compound decoction of aloes combined, and adding, in cases where the catamenia are suppressed and scanty, borax to the mixture; whilst locally, if pain is much complained of, a lotion of acetate of lead, and opium or iodoform ointment (one drachm of iodoform to one ounce of lard, a few drops of rectified spirit being used

to dissolve the iodoform), which is a good anæsthetic, are used. In those who are somewhat more advanced in years, and in whom the disease is both rare and obstinate, the peroxide of hydrogen and tincture of the perchloride of iron are the remedies to be recommended. The dose is a teaspoonful of the peroxide, and ten to twenty drops of the tincture of iron in a wineglassful of water thrice daily. These two medicines, however, cannot be ordered together as in a mixture. A flannel bandage should be applied every morning and noon during the day, especially in the case of old people. (*Dublin Journal of Medical Science*, June 1, 1872.)

Extracts from British and Foreign Journals.

Therapeutic Value and Toxic Actions of Veratrum album, Veratrum viride, and their Alkaloids.—M. Eugène Peugnet contributes a long and interesting paper to the *Medical Record*, on the effects produced by the different species of hellebore, giving the results of an inquiry into which he was led by the occurrence of a case of poisoning with veratrum album in his practice. The hellebores are much more largely prescribed in American than in European practice, and from the history given by M. Peugnet it appears that several different alkaloids, as veratroida (the veratria of Pelletier and Caventon), viridia jervina, and baratyna, have been procured from the roots of the different species by chemists working with different processes. Dr. Peugnet gives the results of 57 carefully watched experiments, in which the pulse, respirations, temperature, and other points were noted. He finds that it is in the resinoid or veratrine of veratrum album that resides the essential and distinct difference between the two varieties of veratrum, and his researches have led him to the following general conclusions:—

I. They confirm the views of Richardson and Scattergood as to the identity of the veratroida in both.

II. They confirm the views of Bullock as to the veratroida's being distinct from the veratria (sabadilla).

III. They confirm the existence of Simon's jervina in veratrum album, and of Bullock's viridia in veratrum viride, and further tend to establish the chemical and physiological identity of both.

IV. They establish that Simon was probably mistaken as to the existence of baratyna in veratrum album; for in exhausting the root a large amount of lime is extracted by the acetic or phosphoric acids, and held in solution in the form of acetate or phosphate, and the addition of sulphuric acid will precipitate it in the form of sulphate, which is insoluble; in case the sulphates are used to precipitate the lime, the acetates or phosphates of ammonia, magnesia, potassa, or soda are formed and held in solution.

V. The veratroida is the active sedative principle of veratrum viride, and Bullock and Wood were mistaken in asserting that it was the viridia.

VI. That the combination of the two alkaloids as they exist in their natural state or artificially combined is the most reliable and decided method of obtaining the therapeutical virtue of either *veratrum album* or *viride*, as shown by the experiments of Scattergood, Percy, and Oulmont, with the resinoid of *veratrum viride*; also Bullock's and Wood's with the combined alkaloids, confirmed by my own.

VII. The essential and characteristic differences between the varieties of *veratrum* reside in the resinoid of *veratrum album*, and the characteristic action of that plant on the alimentary canal is due to it.

VIII. In order to obtain the most active root, great care should be taken in selecting it; the time of gathering it is essential in both, for in extracting the alkaloids from two pounds of powdered *veratrum album*, purchased from a German importer, Dr. P. obtained but two grains of *veratroida*, [not a trace of *jervina*, an abundance of earthy salts, and a resin almost inert. Dr. P. did not find two specimens out of five to yield the same result: this accounts for the discrepancy between the views of such high authorities as Pereira, Christison, and others, and mine, as to the constant effect of *veratrum album*; for Dr. P. maintains that if it has been collected at the proper season, and has not deteriorated by keeping, and is either given in substance or its active principles are administered, it will invariably cause vomiting, purging, great prostration, and gastro-intestinal hyperæmia; in many cases gastro-enteritis will ensue as a secondary result. These effects of course vary with the amount taken.

In case the tincture of *veratrum viride* or its fluid extract is preferred, the rootlets should be separated, as they probably contain less of the alkaloids. As the tincture or fluid extract of *veratrum album* would contain the *veratrine* or resinoid, the obnoxious principle, it would be necessary to extract the alkaloids and make use of them.

IX. In cases where alarming symptoms manifest themselves, the basis of the treatment should consist of stimulants, rubefacients, and opiates. In poisoning by *veratrum album*, the gastro-intestinal hyperæmia should be early overcome by the tincture of *cantharides*, as suggested by Percy in reference to the dilatation of the capillaries, general and local electrification, and the early use of warm enemas to wash away the effused blood and mucus.

X. The presence of the alkaloids can be detected in the urine.

Turpentine in Acute Affections of the Tympanum.—Dr. Friedrich Weber states that he has been in the habit of employing turpentine for two years past in acute diseases of the middle ear. According to his experience, there is no remedy

that equals it in efficacy in these forms of disease. He has prescribed it in every stage of otitis media acuta when pain is present. It supersedes the application of leeches, cold poultices, warm instillations into the ear, though in severe cases they may be employed as aids. The air douche, to remove the accumulated secretion, and the careful cleansing of the external meatus and of the tympanic cavity if the tympanum happen to be perforated, must not be neglected. Weber recommends the administration of the turpentine in relatively large doses twice or three times daily, a teaspoonful being given at a time, followed by a little lime-juice to take away the unpleasant flavour; or the remedy may be given in capsules. In inflammation of the middle ear, accompanied by acute nasal and pharyngeal catarrh, less quantities may be prescribed. He prescribes in such cases inhalations of the vapour of chamomile flour infusion mixed with some turpentine. He also prescribes turpentine as a prophylactic against inflammation in cases where operations on the tympanum have to be performed. He gives the following case:—A young woman who had been exposed to a draught of cold air, suffered from pain in the neck, followed by continuous roaring and singing noises in the ear, accompanied by such intense pain that for many nights consecutively she was unable to obtain the slightest sleep. Cataplasms, blisters, leeches, quinine, opium, and iodide of potassium had been severally tried without effect. Objective examination gave negative results, so that in fact it was diagnosed as otalgia nervosa. Weber prescribed turpentine three times a day, the meatus being simply stopped with charpie. The turpentine made her feel faint, but she slept for the first time for four weeks. In the morning and afternoon she took three capsules containing about fifteen drops apiece, and in the evening four capsules. For some days following she took successively smaller doses, and was soon cured. Some dysuria and diarrhoea were produced. (*Monatsschrift f. Ohrenheilkunde* Jahrg. v., No. 3, and *Medizinisch-Chirurg. Rundschau*, Feb. 1872.)

The Size of the Liver and Spleen.—A. Steffen gives the results of numerous researches on the size of the liver and spleen, in childhood, after death, both in health and under pathological conditions. He finds, as regards the *Liver*—1. That its weight is, in relation to the weight of the body, greatest immediately after birth. 2. That no change in this proportion occurs during the first months of life. 3. That its weight diminishes in proportion to the weight of the body towards the close of the first year—a relation that (4) is more obvious in boys than in girls. 5. As years pass by, the size of the liver continues to diminish relatively to the body-weight, though

both undergo steady increase. Absolutely, no difference occurs between the two sexes in this respect. 6. If the development of the body generally is arrested, the relative size of the liver remains greater than under normal proportions. 7. With imperfect nutrition of the body generally, the absolute size of the liver remains below the normal amount. The same relations hold for the spleen as for the liver. In regard to pathological changes, Steffen finds that scattered accumulations of fat occur in the dead bodies of almost all children. Diffused accumulation of fat is more rare, and the amount must be very great for the liver to show the obtuse edges seen in adults. The highest grade of diffused fatty liver occurs in cases of the so-called pulmonary tuberculosis. Great enlargement both of the liver and of the spleen is always found in the acute exanthemata, in typhus, &c. Febris recurrens is accompanied by very great enlargement. In purpura the spleen and liver enlarge, but in syphilis the liver alone, and only in some cases. Enlargement of the liver is produced by stasis, as in pleurisy, emphysema, and exceptionally in chronic pneumonia. Cardiac disease, especially mitral insufficiency, causes *no* enlargement of either the liver or spleen. The greatest enlargement of the liver occurs in amyloid degeneration, and of the spleen in leucæmia. The liver is not much enlarged in sclerosis, and not at all in tuberculosis of the organ or of the biliary ducts. The spleen in these conditions is smaller, and wrinkled on the surface. The liver is contracted in profuse intestinal catarrh and in cirrhosis. Numerous and carefully drawn up tables accompany the papers. (*Jahrbuch f. Kinderheilkunde*, No. 72, and *Rundschau*, May 1872.)

Treatment of Imperforate Anus.—Amongst the operations that may at any moment present themselves to the surgeon, that required for the relief of imperforate anus is one of the most delicate and important, and he should be prepared to meet any difficulties that may present themselves. "We too frequently neglect," says M. Verneuil, "to ask whether the newborn infant has evacuated the urine and meconium; and when it is ascertained that the anus is imperforate, much valuable time has been lost." Thus, he has himself been called upon to operate on the fourth day. He observes that the success of the operation has been made greater in recent times, when, instead of pushing a trocar at hazard in various directions, deliberate dissection has been made. This is particularly requisite where there is no projection of the rectum, or where the inferior extremity of the rectum is altogether absent. A convenient place should be selected, the infant on its belly, with the knees bent and thighs well separated. An incision should then be made from the easily

found point of the coccyx, along the median raphé towards the scrotum or vulva. It is important to keep in the middle line, where we find always, as we are taught by embryogeny, in the absence of the rectum, a fibrous band which runs as far as to the membranous region or inferior third of the vagina. This is a valuable guide that must not be lost. When the incision made *layer by layer* is sufficiently deep, then may be perceived, on separating the cut edges of the wound well from each other, and directing a jet of cold water upon them, a small black point not larger than the head of a pin. This is the intestine, and if it be moveable it should be drawn towards the skin. It is a fortunate circumstance when this can be done without opening it. More frequently it is only possible to seize the end of the intestine with the hook, and an incision is then made into it. The meconium then flows away, and begins at once to be a source of trouble; its flow sometimes lasts for a considerable period. It must be watched with patience, and waited for till it has finished, in order to complete the operation, which consists in sewing the rectum securely to the skin, taking care that the opening is free, and that there are no chances of retraction or of infiltration. But it may also happen that a deep incision may be made into the perineum, and nothing may be found. The situation becomes a grave one, for it is necessary to continue the dissection into the true pelvis. The operation is difficult, and the guides to it obscure. Not unfrequently the absence of the rectum exists for a considerable portion of its extent. To keep straight in this course across the pelvis, it is important not to lose the walls, the curvature of the sacrum in particular, which is a valuable guide. It is, nevertheless, attended with much difficulty, and M. Verneuil has suggested a proceeding which materially facilitates it. It consists in giving a cut with the scissors on each side of the coccyx, which can then be drawn back, and at once affords greater space to work in. In one instance M. Verneuil found a cut of a quarter of an inch long on either side sufficient, but in others it is necessary to make the incisions nearly a quarter of an inch. He has thus succeeded in cases where otherwise the operation would have had to be given up, and some other attempted. Once formed, the retraction and contraction of the new anus should be prevented by directing the mother to introduce the point of the little finger into it several times a day. Most of the cases of imperforate anus prove fatal. (*Journal de Médecine et de Chirurgie*, tome xliii. part 2.)

Town Cachexia, and its Treatment by Compressed Air.
—Dr. Le Roy observes that deficiency of the red globules and of albumen in the blood is a characteristic of the condition of

chloro-anæmia into which so many of the fixed residents of large towns and cities fall, and that this condition constitutes one of the principal obstacles to the beneficial effects of medicine in the treatment of the various symptoms of which they complain. The nations of antiquity have everywhere left, in large baths, gymnasia, amphitheatres, and circuses, the traces of their attention to the physical requirements of the population. But in the present day no nation appears to bestow any care on the general development of the bodily powers. Dr. Le Roy attributes the pallor and debility of town populations as being on the whole due, first, to want of exercise of the muscles; secondly, to the obscurity and privation of light in which most live; and thirdly, to the close and confined air they breathe. All these conditions lead to an accumulation of carbonic acid in the blood; and he paints in glowing colours the feelings produced by a summer spent on one of the lower elevations of the Swiss mountains. This he considers to be the true cure for town anæmia. But the place of such change of air may be supplied to a certain extent by temporary exposure to increased pressure of the air; and it appears from the careful investigations of Tabarié, of Junod, of Pravaz at Lyons, of Bertin at Montpellier, and of Vivenot at Vienna, that an increase of pressure, amounting to from 1 to 2 centimetres of mercury in the manometer, will give very appreciable signs of improved activity in the performance of the functions of respiration, circulation, &c. The degree of increased pressure to which individuals should be exposed should vary with the age, sex, and constitution, as well as the complaint. The metallic apparatus forms a small circular apartment or tastefully furnished boudoir, in which two persons can remain with comfort. The door is of ordinary size, and kept closed by the internal pressure. Light is admitted by three large windows. The perfect quiet is broken only by the slight hissing of the fresh air entering and leaving, so that sleep can be readily obtained. The augmentation of pressure should be gradually produced, and exposure to it should last an hour or two. By special arrangements the air can either be heated or oxygenated, as occasion may require. The effects produced are that, according to Vivenot, the diaphragm descends permanently during the bath of compressed air half an inch, and the lungs are consequently dilated to a corresponding amount. It is a noticeable circumstance that the lungs do not return to their original volume, but remain persistently dilated. The number of respirations falls, and to a greater extent in proportion to the number previously made, whilst their depth increases; this, also, is a persistent effect. More carbonic acid is eliminated in a given time, and more oxygen absorbed. The pulse falls if it has been previously unduly accelerated; thus in one instance,

the pulse being 110, it fell to 70 during the bath, and remained between 70 and 80 till the following day. The beneficial effects are especially well marked in dyspepsia and gastralgia, when the debility is profound and the emaciation complete; the appetite is increased with the augmented action of the lungs, digestion is improved, and with this the general health undergoes marked amelioration. (*Bulletin Général de Thérapentique.*)

New Method of applying Caustics.—Dr. Aubert observes that the idea of attacking a tumour at its base by means of caustics, or of applying them to the circumference of a limb, is of ancient date. Without doing more than alluding to the earlier attempts, he mentions those only which have preceded his own suggestion, and have adopted a somewhat similar plan, viz. strangulation of the tissues in a flexible caustic loop. Chaissagnac proposed an apparatus which consisted of a series of small rectangular eyes in glass or crystal, disposed in linear series on a bracelet. Into each of these eyes a small quantity of the caustic was inserted, whilst it was prevented from falling out by a slide; the whole was then wound round the part to be removed. Any caustic could thus be easily applied, as the Vienna paste, sulphuric acid paste, paste of Cauquoin, and the caustique Filhol. M. Laroyenne more recently proposed the plan of pushing into the base of the tumour, the removal of which is desired, two long arrows of the pâte de Cauquoin fixed on strips of solid linen, and successively placed, by the aid of a long and large trocar, under the centre of the tumour, so as to form two pedicles, and to attack them both at once throughout the whole of their circumference. Dr. Aubert's plan is somewhat similar: it consists in multiplying the points of attack by employing the usual operative proceedings of drainage for the passage of caustic loops, to favour the action of these by energetic pressure, and finally to limit the action of the caustic precisely by the use of a flexible metallic tubing or gutter in which it is contained. For this purpose he takes a piece of lead tubing about one-third of an inch in diameter, and cuts away a portion of the wall, so as to convert it into a channel long enough for the purpose required. This he fills with caustic and introduces after the fashion of a drainage-tube, into the tissue, and surrounds it firmly. (*Lyon Médical*, June 1872.)

Treatment of Strangulated Congenital Hernia.—At the *séance* of the Académie de Médecine of the 21st May, 1872, M. Demarquay presented to the members a young man, aged 20, on whom he had practised reduced strangulated congenital hernia by aspiration of the liquid and gases contained in the intestinal loop. This patient had spent the day with his family

at Versailles. In the evening, when much fatigued, he was seized with severe colic, accompanied by vomiting, and he noticed that a tumour of large size had formed in the left groin. The pain and vomiting persisting through the following day, he saw a surgeon, who advised him to apply for relief to the hospital. The resident medical officer tried the taxis without effect, and left him till the next morning with a bladder of ice upon the part. The patient had a bad night, being much excited and vomiting frequently. On the following day the patient was much excited and feverish; the hernial tumour was voluminous, elongated, pursuing the course of the inguinal canal. The testis was in contact with the intestine. The case was diagnosed as one of congenital, inguinal, and strangulated hernia of the left side. M. Demarquay, never having succeeded in curing one of these herniæ by operation, first tried to reduce it by the taxis, but without result. He then determined to make trial of the method by aspiration of intestinal liquids and gases. A small trocar was pushed into the centre of the tumour, and the fluids withdrawn by means of M. Potain's aspirator. By this means, in addition to the gases, 120 grammes of fluid were removed. The tumour became completely flaccid. The trocar was then withdrawn, and some minutes were allowed to elapse to see whether new fluid or gas would accumulate in the loop of strangulated intestine. No tumefaction, however, occurring, the taxis was again attempted with great caution. Very slight pressure only from below upwards was required to make the intestine retreat into the abdominal cavity. Opium in small doses was then prescribed, and no further complication occurred except that the testis became inflamed. (*Journal de Médecine*, June 1872.)

Notes and Queries.

DEPARTMENT OF ANALYSIS AND INVENTIONS.

PEPSINE.

(Continued from page 383.)

No. IV. BULLOCK AND REYNOLDS' (3, Hanover Street, Hanover Square) PEPsINE PORCI (DR. BEALE).—Dose, two to four grains. Pale yellow powder, only partially soluble in water. The solution becomes slightly turbid on boiling, gives a moderately dense precipitate with nitric acid and corrosive sublimate, and a very strong precipitate with tannin. No starch present. Extremely slight digestive power.

No. V. BULGOYNE, BURBRIDGE AND CO.'S COMMON PEPsINE.—A very small proportion only soluble in water. Solution gives scarcely any precipitate on boiling, or with nitric acid, and only a slight one with corrosive sublimate and tannin. Much starch present. Extremely slight digestive power.

No. VI. PEPsINE BOUDAULT ACIDIFÈRE, No. 1. *Exécutée d'après la formule du Dr. L. Corvisart.*—Very pale yellow powder, partially soluble in water. Solution remains clear on boiling, gives no precipitate with nitric acid, and only a very slight one with corrosive sublimate; but gives a very strong precipitate with tannin. Much starch present, and 7·1 per cent. of lactic acid. Extremely slight digestive power.

No. VII. PEPsINE WINE PREPARED FROM PURE PEPsINE—T. MORSON AND SON.—Dose, a teaspoonful in a little water, sherry or brandy and water. Scarcely any digestive power.

No. VIII. Sample of pepsine stated to contain one-third of commercial pepsine. Pale yellow scales, partially soluble in water; solution gives no precipitate on boiling, nor with nitric acid or corrosive sublimate, but a strong precipitate with tannin. Starch present; extremely slight digestive power.

Some of the analytical details are given in the following tables:—

TABLE IV.

	No. IV.	No. V.	No. VI.	No. VIII.
Moisture	7.30	11.90	9.70	15.43
Organic matter soluble in water .	24.40	6.00	39.00	43.19
Organic matter insoluble in water	63.00	80.00	48.00	37.90
Mineral matters	5.30	2.10	3.30	3.48
	100.00	100.00	100.00	100.00

TABLE V.

Small cubes of hard-boiled white of egg, digested for three hours and a half with 1 gram. each of pepsine (in the case of the pepsine wine 3 fluid drachms were taken instead), 100 cub. cent. water, and 0.4¹ cub. cent. strong hydrochloric acid, at a temperature of 90° to 95° Fahr. Amount of albumen dissolved at the end of that time by the

Pepsine No.	IV. = 0.086 grms.
„ „	V. = 0.078 „
„ „	VI. = 0.047 „
„ „	VII. = 0.031 „
„ „	VIII. = 0.034 „

TABLE VI.

Thin slices of lean boiled beef, digested for four hours with 1 gram. pepsine, 50 cub. cent. water, and 1 cub. cent. hydrochloric acid, at a temperature of 100° Fahr. Amount of beef dissolved at the end of the four hours, in the case of the

Pepsine No.	IV. = 0.480 grms.
„ „	V. = 0.290 „
„ „	VI. = 0.120 „
„ „	VII. = 0.140 „
„ „	VIII. = 0.380 „

The amount of substance digested in a given time depends, irrespective of the amount of pepsine present, on a great variety of circumstances; such as the temperature, the amount of acid and water, the mechanical condition of the substance to be digested, &c. &c. It is therefore impossible to determine the absolute amount of substance which a given proportion of pepsine will digest. However, under conditions otherwise equal, two mixtures containing the same quantities of pepsine will digest the same quantities of substance in equal times, and this enables us to compare two different samples of pepsine with each other. A solution of pepsine, to serve as a standard, was accordingly

¹ In Table II. 0.4 cub. cent. should stand instead of 0.2 cub. cent.

prepared as follows, from some fresh pigs' stomachs:—The pyloric end having been cut off, the glandular layer was detached from the rest, washed, soaked in water for twenty-four hours, after which it was cut up small and digested for three days in pure glycerine. The mixture was then filtered, and the clear solution taken for experiment. Analysis showed that 1 cub. cent. of the glycerine contained, at most, 0·015 grms. pepsine.

TABLE VII.

Small cubes of hard-boiled white of egg were digested for three hours and a half, at a temperature of 90° to 95°, with 100 cub. cent. of water, 0·4 cub. cent. of strong hydrochloric acid, and $\frac{1}{2}$, 1, 2, 4, and 8 cub. cent. of the above glycerine solution respectively. Amount of albumen dissolved at the end of that time in

No.	I.	= 0·050 grms.
„	II.	= 0·067 „
„	III.	= 0·092 „
„	IV.	= 0·100 „
„	V.	= 0·117 „

TABLE VIII.

Thin slices of lean beef digested for four hours, at a temperature of 100° Fahr., with 50 cub. cent. water, 1 cub. cent. hydrochloric acid, and $\frac{1}{2}$, 1, 2, 4, and 8 cub. cent. respectively of glycerine solution. Amount of beef dissolved in

No.	I.	0·360 grms.
„	II.	= 0·41 „
„	III.	= 0·51 „
„	IV.	= 0·66 „
„	V.	= 0·77 „

Now, taking into consideration only the experiments on albumen, since these, owing to the much greater uniformity of character in the substance employed, are the most likely to give good comparative results, we arrive at the following proportions of real pepsine contained in the various preparations examined:—

No.	I.	less than 0·5 per cent.	No.	V.	about 2·2 per cent.
1,	II.	about 6·0 „	„	VI.	„ 0·7 „
„	III.	„ 2·5 „	„	VII.	less than 0·2 „
„	IV.	„ 2·6 „	„	VIII.	about 2·5 „

These figures must of course be looked upon as approximately correct only, or rather as giving the maximum amount of real

¹ With 1 gm. pepsine No. II. 0·1 gm. of albumen was found to have been digested (Table II.); to produce the same effect four of the glycerine solutions were necessary (Table VII.). These four contained 0·06 gm. of pepsine, and the like amount must have been contained in the 1 gm. pepsine employed, since the same effect was produced; in other words, the pepsine employed contained 6 per cent. of real pepsine. The other figures are calculated in the same manner.

pepsine which could have been contained in the different preparations examined.

NEW ELECTRICAL APPARATUS.—Messrs. Meyer and Meltzer have submitted to our inspection an exceedingly ingenious apparatus, which, in small compass, contains a complete constant battery of twenty very powerful cells (Stöhrer's), and a powerful induction machine. So far as we can judge, this invention will provide the medical practitioner with a perfectly portable armamentarium, complete for all practical purposes, both of diagnosis and treatment, to which electricity can be applied in medicine. The cost is comparatively moderate.

Messrs. Meyer have also shown us a small induction machine, exceedingly handy of application, and little likely to get out of order. The single-celled battery is carbon and zinc made active with sulphuric acid and bichromate of potash.

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¹ Any of the foreign works may be procured on application to Messrs. Dulau,
of Soho Square, W.C. ; Williams & Norgate, of Henrietta Street, Covent
Garden, W.C. ; or Baillière, of King William Street, Charing Cross.

THE PRACTITIONER.

SEPTEMBER, 1872.

Original Communications.

ON A CASE OF "WRITER'S CRAMP," AND SUBSEQUENT GENERAL SPASM OF THE RIGHT ARM, TREATED BY THE JOINT USE OF THE CONTINUOUS GALVANIC CURRENT AND THE RHYTHMICAL EXERCISE OF THE AFFECTED MUSCLES.

BY G. V. POORE, M.D. (LOND.), M.R.C.P.

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I FEEL that no excuse is needed for publishing the following case, for not only is it a curious sample of a most rare and interesting disorder, but the fact that it has proved amenable to treatment after a duration of nine years, makes it almost, if not quite, unique. It is interesting also as illustrating a novel and successful mode of employing one of the most valuable therapeutic agents which we possess. The history of the case is as follows:—

George Gair, aged 32, single. A small man, well made, rather muscular, and of very healthy appearance. No history whatever of any hereditary neurotic tendencies. Has always enjoyed most excellent health. He has always been steady, and, while able, was hard-working and industrious; has never had syphilis.

He is a well-educated and very intelligent man. He left school when fourteen years old and became a writing clerk in a commercial house in the City. He wrote an excellent hand, and gained rapid promotion in his office. In the year 1859 he left his situation and entered an accountant's office, where he did, on an average, nine hours' writing a day. By reason of his neat style of writing he was constantly employed in copying balance-sheets. In this situation, in which he remained for fifteen months, he earned five pounds a week, and there can be no doubt that the amount of writing which he was called upon to do was something prodigious. He left his situation because of his present illness, which came on almost without warning of any kind. He had been working as well as ever, when one day, towards the close of his day's work, he says, "he had a difficulty in bringing his right hand down upon the paper." He managed, however, for that day and for the three following days to continue his writing by holding his right wrist firmly on the desk with his left hand. At the expiration of three days he found himself wholly incapable of accomplishing the neat work required of him, and he had to leave his employment.

For about a month subsequently he managed to write a few letters by steadying the palm of his hand against the edge of the desk and only using his fingers. Between that date (October 1862) and January 1872 he has been totally unable to accomplish any writing at all with his right hand. He gradually acquired the art of writing with his left hand, and what writing he has absolutely been required to do has been accomplished in this way. During last year, however, the left hand began to suffer from cramps and ultimately became as stubborn as the right. His means of subsistence being taken from him, he was obliged to live upon his savings, which were considerable; but his store of money gradually diminished; and about the middle of last year he began to experience real want and to be filled with apprehension for the future.

About this time, and probably as an effect of his straitened circumstances and anxiety of mind, his right hand, which hitherto had refused to write only, began to be affected with spasms at other times. He gradually had lost the power of

using his knife at dinner, and occasionally he found himself unable to accomplish the most ordinary acts by reason of sudden spasms of the muscles of his right arm. Shortly before I saw him he had broken the jug belonging to his wash-hand basin, in consequence of a sudden spasm just as he was about to pour out some water.

During the six weeks previous to his coming under my observation there had been a further exacerbation of his condition, and the right arm had become liable to sudden spasms even when not called upon to perform any act. It was the seat of an exaggerated local chorea; it was always jerking about, and at times would bounce out of the side pocket of his coat as he was walking in the streets. These strange antics naturally attracted attention, and immediately the patient saw that he was observed the spasms became doubly severe. In January of this year he came under my notice at the out-patient department of the Charing Cross Hospital, and the following account of his then condition is taken verbatim from my note-book:—
“On asking him to strip himself to the waist, he does so without difficulty. There is no evident impairment of the nutrition of the muscles of his right arm. The right arm hangs by his side and is subject to constant twitchings. The deltoid, the pectoralis major, the scapular muscles, as well as the biceps and triceps, are all affected; but the last-named muscle is the worst offender, and in it the cramps are more constant and more severe than elsewhere. While I am talking to him the arm is forcibly extended, and the triceps is as tense and hard as a board. The fingers and wrist are often flexed, but never extended. The thumb and fingers do not seem liable to spasms individually. At times the spasms subside for a few moments, but any allusion to them seems to bring them back.” [This apparently was due to nervousness, and was exactly analogous to the extra difficulties experienced by a stammerer when attention is called to his defective speech.] “On being requested to perform any act, the right arm ‘jibs,’ as it were, like a stubborn mule, and it is only by main force and by the greatest concentration of thought and determination that the most simple things can be accomplished. Everything is done with the arm extended. On being asked to unlock a box which lay on the table, the arm was stiffly extended,

and the patient, standing of course at arm's length from the box, managed with great difficulty to unlock it.

"Of all acts, writing occasions the greatest amount of spasm. On asking him to write his name, he takes the pen in hand, and immediately he does so there is a violent cramp of the triceps; the arm is forcibly extended, and with great difficulty he manages to write 'Geor' in a manner scarcely legible, when the hand is twisted off the paper by a violent rotation of the wrist, and his fingers lose the grasp of the pen. On asking him to continue writing, he is perfectly unable to do so, and every effort even to place his hand on the paper seems to be violently resisted by every muscle from the deltoid downwards.

"The spasms of the arm never come on during sleep. He states, however, that latterly, owing to mental worry, he has slept very badly."

So much for the history of the case and the condition of the patient when he first came under observation. At the beginning of July last he furnished me with an account, *written by himself*, of the various treatments to which he has been subjected between 1862 and the present time. I give it in his own words.

"1. Galvanism three times a week and manual labour, such as digging, &c. Was under this treatment for about six months, and derived no benefit.

"2. Sea-bathing, cold water pumped or poured on the arm about every two hours, exercise with the dumb-bells, rowing, gymnastics, &c. Was under this treatment for about three months. No benefit.

"3. I was now advised a sea-voyage, but instead of taking one I obtained a situation as steward on one of the boats running between Bristol and Cardiff. I tried this about three months, but no improvement.

"4. Galvanism twice a week, and one of Pulvermacher's chain bands worn round the arm. I was also to use the arm as much as possible. I was under this treatment twelve months, but got gradually worse." [He has a couple of scars on his right arm, the result of a burn by Pulvermacher's chain band.]

"5. Cold shower-baths and friction with horse-hair gloves every morning, and the spine rubbed with croton oil three or

four times a week. I was considerably worse after twelve months of this kind of treatment. I have also had Measam's cream and various oils rubbed into the arm at different times."

This account of himself is accompanied by fac-similes of his handwriting at various times between January 20th and the present date, so that by a reference to it the reader may see how gradual, but at the same time how continuous and satisfactory, the improvement has been. The man himself is an excellent patient, and has followed out with the most scrupulous care and dogged perseverance every suggestion which has been made to him.

The published cases of "Writer's Cramp" and the articles upon the subject in our text-books gave one very little hope of bringing about any improvement; but as such cases are of a peculiarly interesting nature, I resolved to try every therapeutic agent which seemed in any way applicable. First, small doses of strychnine were given, but as the patient said very decidedly that the medicine made him worse, they were soon discontinued.

Mental irritation and distress and sleeplessness being marked features, bromide of potassium was given and with excellent results, for it procured good nights' rest, and seemed to diminish a little the spontaneous spasm of the arm. This seemed to be due to his paying less attention to his condition. His power of writing showed no improvement after the bromide alone; but in consequence of the sleep and mental ease which it seems to give, it has been continued in doses of fifteen grains three times a day throughout his whole course of treatment.

From the very first *rest* was enjoined. In order to make quite certain that the arm could be used for nothing at all, I first attempted to put the whole of the arm in an immovable apparatus of plaster of Paris, but this did not succeed, for the spasms were so violent and constant that it was impossible to get the plaster to "set" properly, and very soon the rough ridges which formed in the bandage began to bruise the arm. The plaster of Paris was therefore removed within forty-eight hours of its application, and the patient was merely ordered to keep his arm in a sling. To this he adhered rigidly for two months or so, and then, the general spasm having much diminished, he was allowed gradually to use his arm more and more, and at the

present time he is allowed to use it as much as he likes for every purpose except that of writing.

I first saw the patient on January 20th, and for the first three weeks the above methods of treatment were alone employed.

On February 8th, in addition to the bromide of potassium and rest in a sling, I commenced the use of the continuous galvanic current. At this date he had improved somewhat in so far as his condition bothered him less, and he always slept soundly at nights. The general spasms of his arm were still as bad as ever, and his inability to write had not in the least abated. The improvement in his mental condition, which served as it were as a solid foundation for his further treatment, was due, in my opinion, entirely to the bromide of potassium, which he still continues to take.

The galvanic current was used in a peculiar way, and, as a reference to the accompanying fac-similes of the patient's handwriting will show, with the best results.

A slight explanatory digression here becomes necessary. In searching for a cause for this man's condition I was completely baffled, so that it seemed useless to proceed as in other diseases and attempt to remove it. There was no indication of any disease of the spinal cord, such as Mr. Solly, in his paper on this subject, hinted at as the probable cause of the cramps. The man, with the exception of his right arm, was perfectly well. I could find nothing wrong with him. His digestion was excellent, and he was capable of any amount of physical exertion. With the exception of the spasm, I could find nothing wrong with his right arm. There was neither hyperæsthesia, paræsthesia, nor anæsthesia of any part, and no paralysis of any of the muscles. The spasms themselves did not point to any particular nerve at fault. All the muscles of his right arm and shoulder were in a state of revolt, refusing to obey orders, and the worst offender and ringleader appeared to be the triceps extensor.

The case seemed to me to be analogous to stammering. This man stammered with his right arm; and just as ordinary stammerers while they haggle over everything find special difficulty with certain words, so this man's arm, while it jibbed as it were at every orderly action, was especially stubborn when called upon to write.

Now, every stammerer that I ever have met can sing. They are all capable of a rhythmical use of the voice, and every stammerer has, I believe, his cure within his own grasp if he persevere in the orderly and rhythmical exercise of his vocal powers. Regular, orderly, continuous and rhythmical *drilling* converts as it were his "awkward squad" of muscles into obedient and well-disciplined servants of his brain. I determined to apply the above principles to the treatment of a stammering right arm.

It was necessary, however, to get rid of the spasms by artificial means before the rhythmical exercise could be commenced. For this purpose one of Weiss's continuous current batteries was employed, and twenty-three cells were used to begin with. The sponges, being well wetted with salt and water, were first placed on either side of the belly of the deltoid muscle (which at the time was the seat of almost continuous spasm). The spasm immediately subsided, and then the man was made to exercise his deltoid while I counted "one, two," "one, two," like a drill sergeant, every time he elevated or depressed his arm, the patient keeping time to the counting.

Other muscles were then exercised in the same way—the pectoralis major, biceps, triceps, pronators, supinators, and the flexors and extensors of the wrist and fingers, care being taken not to overtire the muscles. Every possible kind of rhythmical exercise has been gone through. The pectorals have been exercised by practising "extension movements" and drawing back the shoulders, and the fingers and thumb have been also drilled by opposing the thumb to every finger in turn, and by making him run along the mantel-piece or the table with his fingers as if playing the piano. It is unnecessary to detail the way in which the galvanic current was employed for every muscle or group of muscles, but the samples quoted will serve to show the principle. The good effects of this plan of treatment were soon manifested. It was commenced on February 8th. On the 9th the patient stated that "his arm had been remarkably quiet since the electricity yesterday;" and on the 11th he said that he had already derived so much benefit that his arm was no longer a nuisance to him, but, on the contrary, he was able to use it for dressing himself; and on the 12th he buttoned his

collar, which he had not done for months before. A reference to the lithographed fac-similes of his handwriting will show that this too began to improve in a most remarkable way. The galvanism and rhythmical exercise has been continued every day without intermission from February to the present time, and the patient still continues to make gradual and marked improvement. He can now accomplish everything, except writing, with perfect ease, and even the power of writing has improved in a very great degree. He has written me two or three letters, and has been able to embark in a small business. When he writes, he says, he feels like a schoolboy beginning to learn, and the act of writing is gradually becoming more easy to him. The great bar to his writing hitherto has been the triceps muscle, which, till recently, has taken on a spasmodic action whenever he has taken a pen in his hand; and all his writing has been hitherto accomplished with the arm forcibly extended. This condition of the triceps is being gradually overcome, and the 8th sample of handwriting was written "with the greatest ease, and with the arm bent."

In addition to the galvanism he has had a daily hypodermic injection of morphia (gr. $\frac{1}{8}$). I do not think that this was of any decided benefit to him. It has been discontinued now for the past six weeks, and his improvement has been, I think, more rapid since. At the beginning of June the galvanism was discontinued for a fortnight while the battery was under repair, and it was gratifying to find that during this time he suffered no retrogression.

I append the dates and a few remarks concerning some of the specimens of handwriting.

1. Written on January 20th, 1872, before the adoption of any treatment.
2. Written on February 8th, after first use of the galvanic current.
3. Written on February 9th. The word "Gair" was written while the sponges were held on the front and back of his fore-arm.
4. February 14th.
5. Middle of March.

1. Geo

2. Geo

3. George Geo

4. George Geo

5. George Geo
Stunnington Park

6. George Geo

77 Lorraine Street

Stunnington Park

April 19th 1872. London S.E.

7. The flower garden just now keeps
us on the move, for everything
is blooming

8. George's Hair Commercial Road

9. The flower garden just now keeps us on
the move for everything is blooming

July 31st 1892

6. April 19th.
7. July 6th.
8. July 25th. Written with the arm bent and "with the greatest ease."
9. July 31st.

I have to express my thanks to my friend Mr. Horatio Symonds for kindly undertaking the care of this patient during my absence from home.

The above case differs from most of the recorded cases in the fact that the condition seems to have consisted solely in a perversion of function. In almost all published cases of writer's cramp we are accustomed to read of "pricking sensations in the wrist," "numbness," and neuralgias, &c., as preceding or accompanying the special spasm. In this case there was nothing of the kind, and neither was there any paralysis nor sign of impaired nutrition of any of the muscles. It seemed, as I have said, to be exactly analogous to stammering, in which, though we have very great functional disturbance of some of the muscles concerned in vocalisation, we do not usually obtain any evidence of pathological changes. I was so struck with the analogy between this case and an ordinary case of stammering, that I have been trying the effect of combining galvanism and rhythm in the treatment of stammerers; but my experience has been too slight as yet to draw any conclusions as to the advantages of such a treatment for stammering, and one great reason for this has been that every stammerer can speak very well if he will only take the trouble. Even the very worst cases that I have encountered can recite prose or poetry if they will do so slowly, and at the same time alter either the pitch or rhythm of the voice, and it is therefore difficult to determine whether or no a weak galvanic current is of any use. One gentleman who stammered very badly told me, however, that he thought he spoke more easily when the sponges were placed beneath the lower jaw on either side of the tongue muscles. "It seemed," he said, "to support and help him."

A CURIOUS CASE OF FACIAL NEURALGIA WITH UNILATERAL SWEATING CURED BY THE APPLI- CATION OF ACONITE LINIMENT.

BY SYDNEY RINGER, M.D.

MARY RANCHEN, aged forty-five, applied at University College Hospital on April 26th. Her father died of rheumatic fever; her mother in her third attack of apoplexy; and eight of her mother's sisters of "apoplexy" at the age of twenty-four or twenty-five. One sister died of dropsy, another of paralysis, a brother of convulsions when a few months old. One brother is a healthy man. She has a son aged sixteen, and a daughter thirteen, both healthy, and who never suffered from convulsions. At her last confinement she had twins, who died immediately after their birth. Five years ago the patient was seized with hemiplegia of the left side, affecting the face as well as the arm and leg. The face soon recovered, but the leg did not recover for two years. The arm recovered before the leg. The catamenia ceased about a year ago. She has felt a little pricking pain on the right side of the face for two months, and meanwhile has suffered from unilateral sweating of the right side, and has passed a large quantity of water, to the extent, she says, of six pints a day, and complains of distressing thirst. Unfortunately we could not get any of her urine to search for sugar.

About eight days ago the right nostril began to run freely, and on the 23rd (three days ago) the right eye began to water, and in a few hours she was seized with a very severe pricking pain of the right side of the face. The pain has recurred three

times daily—in the morning, afternoon, and evening—each paroxysm lasting about two hours. She describes the pain as being most severe, and compares it to pricking every part of the skin with a red-hot needle. The pain corresponds to the distribution of the supra-orbital, infra-orbital, and inferior dental branches of the right fifth. The pain reaches exactly the middle line of the nose, but not quite to the middle line of the upper and lower lips. On April 24, the second day of her illness, the right side of her face became greatly swollen, the swelling increasing with each paroxysm, and decreasing shortly afterwards. On the 25th she felt pricking pain inside her nose. On her first visit (26th), the right side of her face was very red and greatly swollen, so that she opened the eye with difficulty. The right eye was greatly injected, and exposure to the light produced great pain. There was great lachrymation, and its vision was greatly impaired. There was a little swelling on the left side, in the neighbourhood of the infra-orbital foramen. The pain which has been described was most severe, and was limited to the right side; the swelling of the left side of the face being free from pain. She complained of severe pricking pain in the right eyeball. The motor branch of the fifth was unaffected. Hearing and taste were natural. The pupils were much contracted, but equal. There was no ptosis, no strabismus, nor paralysis of the seventh or ninth nerves. The right side of her mouth seemed dry to her, although on the left side there appeared to be excess of saliva. After the attack of paralysis she suffered for some time from cold feet, but gradually this symptom got better, but returned with the present attack. The right extremity is very cold as high as the knee, and is colder than the left. The right is so cold that it wakes her, and she is obliged to put on a thick woollen sock and use a warm bottle. The unilateral sweating on the right side reaches only as low as the knee.

On the day of her first visit we painted the right side of her face with aconite liniment, and on the following day (April 27) we found that the pricking pain was very much less severe and more limited, and the right eye was much less bloodshot, less painful, and she could see distinctly. The running at the right nostril was greatly lessened, but the excess of sweating on the right side was undiminished. The aconite liniment gave her

sharp pain like that of a drawing blister, and made the skin quite numb.

On the 29th she stated that, on leaving on the 27th, slight pricking pain returned under her right eye, but on applying the liniment lightly, the pain in ten minutes disappeared. In the afternoon she experienced slight pricking along the right lower jaw, but the liniment lightly applied removed it immediately, and ever since she has been free from pain. To-day (29th) there is no swelling of her face, the right eye is very little bloodshot, and the sight is perfect. She perspires much less, and the sweating of the two sides is equalised. On the first day after the application of the aconite liniment her feet became warm, and have not since been cold.

[I take the liberty to append a few remarks, for which Dr. Ringer is in no way responsible, to this very interesting case. The above narrative appears to me to afford an instance, though a rare one as to degree, of successful treatment by interrupting the conductivity of peripheral nerve-branches. A multitude of circumstances, which it is needless to point out in detail, indicate that the morbid process which really caused the neuralgia in this case was seated in the medulla, involving primarily the centres which belong to the sensitive and vaso-motor fibres of the fifth nerve: but ultimately spreading to other vaso-motor centres. Yet the cure seems to have been produced simply by paralysing the peripheral portion of the fifth nerve, and thus giving the centres rest from the impact of impressions from without.—F. E. ANSTIE.]

QUININE AND THE COLOURLESS BLOOD-CORPUSCLES.

BY DR. C. BINZ,

Professor at the University of Bonn.

It was agreeable to me to see, from the carefully elaborated paper of Dr. Geltowsky,¹ that he confirmed my researches of the year 1867 in the principal things. The final remark of the editor gives me occasion to make a short reply.

The result of that year's researches is to be found amongst other explanations in the following passage:²—

“In two thousand parts of serum is dissolved one part of thoroughly neutral hydrochlorate of quinia, and of this a drop is added to a fresh drop of human blood. This preparation shows the following condition:—The red corpuscles do not exhibit any change. The white ones, without exception, assume a coarse granular aspect, of course supposing that no spots or stripes of the blood remained unmixed with the serum. Some of them remained persistent nearly in the same form as they had just taken from the addition of the quinine. Most of them became round, but at the same time swollen as they would be from the addition of pure water. Many are plainly divided into two dissimilar halves, one granulated very dark, the other clear and light. Whether the latter represents the puffed up hyaline nucleus, I do not venture to decide. Once I saw most plainly how the hyaline half forced itself to the outside and remained hanging on, a yawning opening of the remaining cellular substance.

¹ *Practitioner*, June 1872.

² “*Archiv für mikroskop. Anatomie*,” p. 383. Bonn, 1867.

"If we now continue to heat this preparation, we shall see that even with a higher temperature than the one which at the beginning caused creeping movements, there is now no sign of life to be noticed. The white corpuscles remain partly quiescent, partly they are forced onward or moved to and fro by the flowing of the red corpuscles, as their power of adhering is weakened or destroyed. *The latter slight passive movements may sometimes be taken for amœboid.* The white corpuscles are never perfectly round, least of all when first heated, and then the poisonous agent has worked upon them, and so a slight passive change of their position easily produces a remarkable change in their outlines."

I compared quinine also with hydrochlorate of morphia and nitrate of strychnia. The former is only a weak poison for the white corpuscles, the latter seems to be equal to quinine, but does not surpass it.

One of my most able students later examined several other substances with regard to the same quality, viz. conia, veratria, camphor, bichloride of mercury, arsenite of potassium, aconitine, digitaline, coffeine, sulphate of atropia, acetate of lead, kreosot, and oil of turpentine.¹ He found that conia alone was equally poisonous to the white cells as quinine, but all other substances were inferior to it. I can, however, only report these experiments, as I did not superintend them.

Later, Kerner added the observation that coffeine, salicine, sulphate of atropia, and arsenite of potassia are not at all or much less poisonous. Geltowsky affirms the same of methylsulphate of strychnia, urea, and chloride of sodium, in comparison to quinine.

The one point on which Geltowsky differs from myself and Kerner is the degree of toxic strength of quinine. We both give as a round number for the human blood the proportion of 1:4,000,—Geltowski only 1:2,800.

I need have no doubt about my number. In my researches the colourless blood-corpuscles *were and remained dead* after a careful addition of quinine of 1:4,000,—in those of Kerner likewise. It can only be admitted that either great individual differences exist, or that slow passive movements were taken

¹ "Chinin als Antiphlogisticum." Dissert. inaug. Bonn, 1867.

for active ones. From his own experiments, my first adversary in this question also admits the correctness of the figures of 1:4,000.¹

In order to give a further experimental contribution to this point, I made use of the opportunity which a case of leukæmia offered me.

A young soldier had been suffering since the siege of Paris from a swelling of the spleen, which at the time I saw him was thirty centimetres long and ten centimetres broad. By help of a fine capillary tube I twice drew from the finger a certain amount of blood. To the one I added an equal amount of serum, to the other the same serum with hydrochlorate of quinia (of very little alkaline reaction) in the proportion of 1:5,000, well mixed with the blood,—thus quinine to this is as 1:10,000. The effect was this:—

Five minutes after.—Blood with serum alone:—Great quantity of white cells. Heated on M. Schultze's object table to 30°--35° C. Very active movements.

Blood with serum and quinine:—Quantity of the white cells of course the same, but many of a darkened aspect. Number of moving ones about the same as in the former preparation, but heated at 30° to 35° C., the energy of many *seems* to be less.

Three hours after.—The preparations had remained protected against any evaporation in a room of 16° C.

Blood with serum alone:—Almost everything as before. Great activity everywhere when heated to 40° C.

Blood with serum and quinine:—The number of the colourless corpuscles which show movement when heated to 40° has decidedly decreased. The movements still existing go on slowly. Most of the cells are round, granulated, sharply defined, their nucleus clear.

Six hours after.—Blood with serum alone:—Decrease of the number and energy of the movements. About the half still alive when heated.

Blood with serum and quinine:—Only a very small minority still alive when heated to 40° C.

I have repeated this experiment with the fresh blood of a

¹ "Deutsche Klinik," p. 100; Berlin, 1869. Cf. my reply in the same periodical, 1869, p. 153.

healthy young dog, with only the difference that I dissolved the weak alkaline hydrochlorate of quinine in 2,500 parts of distilled water, and added it to three times as much blood, which gave the same proportion as before, viz. 1 : 10,000. The result agreed so well in all the principal points with the former, that I consider it superfluous to give the particulars. Besides, it is perfectly immaterial for the essence of the question, whether we employ three or four hours in destroying the white corpuscles, whether we extract still some weak movements by heating, and whether we determine to a fraction the proportion of the poisonous quantity of quinine.

It is evident from these last experiments that quinine is a poison for the white blood-cells of warm-blooded animals even when diluted beyond 1 : 4,000, and, further, that the paralysing influence may already have begun to act, while the movements on the heated microscope have the appearance of integrity.

At present it is impossible to notice with our instruments the lesser differences in the energy of the movements. They may exist without being perceived. The full perception of the accelerated decay of the cells makes up for it.

To steer clear, I have from the beginning struck into a second path. I calculated thus:—If quinine, not in poisonous doses, is a poison for the colourless blood-cells, it must within some hours diminish the number of these elements—even if their movements seem to us active and normal—just as certain stimulants, or for instance a good dinner, increase that number. This in fact showed itself. The proof may be found in my essay about quinine (Berlin, 1868), reported in the *Lancet*, 1868, p. 415, and in the papers of two authors personally unknown to me, Winther and Martin, referred to in Virchow's *Archiv*, tom. 47, p. 159, and in the *Deutsche Klinik*, 1869, No. 17. Finally, Mosler proved the same for the living man,¹ after Briquet had, on the basis of clinical observations, already written down the sentence: "Où l'on veut entraver un travail

¹ "Die Pathologie und Therapie der Leukämie." Berlin, 1872.—Mosler remarked, after repeated injection of quinine, a regular decrease of the spleen, even if all the nerves leading to it had been severed. He thence concludes (p. 256) that the action of quinine takes place without the concurrence of the central organs, and supposes that the contracted fibres of the spleen are directly acted upon. For this latter supposition there is no further foundation.

imminent de suppuration on doit donner la quinine à des doses de 2 à 4 grammes par jour.”¹

Geltowsky thinks, “in order to obtain the special effect of quinine on the white blood-corpuscles of a man, it would be necessary to take almost one drachm, which would be impossible.” To show that this is really possible, I will cite a new example. Dr. Socin, Professor of Surgery in Bâle, who during the Franco-German war directed a large hospital at Carlsruhe, writes the following:²—

“For internal treatment I found large doses of hydrochlorate of quinine very useful; only in high fever six to seven grammes must be used daily;³ and the largest doses must always be given in the morning. Each time a perceptible fall of temperature takes place; but this effect is only a passing one, if one does not continue to give this drug a few days till a decided improvement of the other symptoms also takes place. The results are often particularly striking in perpetual sept hæmic fever. In several severe cases treated thus I saw unexpected improvement and recovery. The use of stimulants at the same time supports the effect and hinders the intoxication by quinine from going too far. We had fever patients who within six to eight days used as much as thirty grammes of quinine, besides drinking three bottles of strong wine daily.”

One drachm is about equal to 3·75 grammes. Further, there is no doubt, that all quinine does not leave the human system in less than twenty-four hours. According to this, the last-mentioned objection speaks more for my theory than against it.

The experiments on the mesentery of the frog show a decrease of the inflammation by quantities of quinine which leave the other organs intact.⁴ The influence on the formation of matter, however, does not assert itself at the beginning.

¹ “*Traité thérap. du Quinquina*,” p. 590. Paris, 1855.

² “*Kriegschirurgische Erfahrungen*,” p. 27. Leipzig, 1872.

³ In this respect, also, much abuse has been committed. Many practitioners are accustomed to pour, *in the form of powder*, the indigestible sulphate of quinine into the diseased stomach of fever patients. Then they are surprised at seeing dyspepsia and sleeplessness. Also the adulterations with the much less effective cinchonine have a great deal to do in these cases, where the supposed quinine remains without any effect. Cf. Virchow's *Archiv*, tom. li. pp. 24–29.

⁴ Cf. “*Archiv für Physiologie*,” Tab. ii. Figs. 5, 6, and 7. Bonn, 1870.

If the cells are once present in the vessels, they have mostly still the power of emigrating. But the number of new ones that follow diminishes; the thick pavement of the inner wall crumbles away more and more, while there is full integrity of the circulation, if one works carefully,¹ and a decided diminution of the formation of pus takes place.

I do not mean to say that with human beings this effect can be obtained quite as quickly and as plainly.² On the contrary, my doubts about it existed long before those of Geltowsky.³ On the other hand, it would be difficult, after all the experimental results, to deny that the acknowledged specific poisonous power of quinine on the colourless blood-corpuscles does not come into consideration when their number or activity pathologically has increased. That, besides this, other effects of the quinine in diseases also assert themselves, I mentioned again in my last article.⁴ Quinine bears various relationships to the different parts of the animal body. The influence on the whole must be a remarkable one, even though each single effect be but small.

If we still see the movements in the blood of an animal poisoned by quinine, we can, in reference to all other facts, as little conclude that quinine has no direct effect on the energy of life of the colourless corpuscles, as we can conclude from the slow or negative influence of moderately strong solutions of carbolic acid on moving bacteria, that this acid is no direct poison to them.

To kill all organisms in putrefying liquids, one must apply a solution of at least one per cent. carbolic acid, whilst there is not the slightest doubt that a much smaller quantity prevents

¹ I particularly call attention to this, because this point also led an author into errors of conclusion. Cf. "*Berliner klinische Wochenschrift*," 1872, No. 16, where Dr. Scharrenbroich gives a reply.

² Geltowsky says: "Kerner has made the experiments only with blood of cats, and the conclusion deduced from these experiments can be valuable only for those animals." But on the very following page of Kerner's paper is mentioned that he also examined the blood of dogs in the same way, and "in a striking manner," with the same effect. And in his former standard paper about quinine, in the very same periodical (1870, pp. 93-165), if even with smaller proportions (1 : 1000), the question of the human blood (woman) is clearly discussed.

³ In my Essay of 1868, p. 58. Berlin.

⁴ "*Berliner klinische Wochenschrift*," November 1871, No. 48.

the formation of bacteria and the energy of their fermenting power.¹

It never can be our object to kill the colourless blood-corpuscles in the human body by quinine. They are necessary for life, and their death would be our death. All we intend to do to them is to decrease their number and their energy, and we can do both these things by quinine in a direct way. The doubts which might arise respecting this subject are experimentally refuted in my former papers, and I must refer the reader to them as regards the details.²

¹ Cf. Plugge, "*Archiv für Physiologie*," 1872, p. 541; Burdon-Sanderson, "*Thirteenth Report of the Med. Off. of the Privy Council*," 1871, App. No. 5; Steiner (Billroth's Clinic), "*Wiener mediz. Wochenschrift*," 1872, p. 711.

² Cf., too, Virchow's *Archiv*, tom. xlv. p. 129.

RACHITIS AND MINERAL INANITION.

BY DR. BLACHE (OF PARIS).

IF there be any term more than another that shows how fully Trousseau was in the right when he set himself against the use of denominations too significative or based on the observation of a single symptom or on transient pathological theories, it is that of Rachitis.

Where is the physician of the present day who would attach to the deformations of the spine, and which, besides, are not invariable, the importance which appears to have been ascribed to them by early observers ?

Far from such being the case ; osseous injuries, and on that point all seem now to be agreed, instead of being considered as constituting the whole malady, are now looked upon as only the natural consequence of defective nutrition affecting at one and the same time every part of the organism.

However, although it is now demonstrated that this disease is but one of the forms under which we observe the results of mineral inanition, we think it should preserve its name of rachitis or the rickets, as it is in common use, is understood by all, and is easy to be retained.

We have just pronounced the name of mineral inanition ; within the last few years, a certain number of physicians, and M. Gubler especially, have rightly brought under this head rachitis and a certain number of morbid states in which nutrition is defective.

None, however, appear to us to have sufficiently set forth on the one hand, the connexion existing between the defective nutrition and the osseous injuries caused by it ; and, on the

other hand, the cause of the differences observed between the various manifestations of the same state; we will, therefore, endeavour to elucidate these two points, and ascertain how far the results of therapeutics lend support to our views.

Let us first revert to what has been recently demonstrated in a work by M. Dusart, "*Archives Générales de Médecine et de Chirurgie*," 1869-70. This author has concluded from previous labours and his own experiments:—

1. That, in all living beings, whether vegetable or animal, the presence of phosphate of lime is necessary for the transformation into cells of the azotized matters supplied by food, and that, to preserve their vitality, the tissues must be constantly traversed by a kind of current of phosphate of lime.

2. That the vital activity and temperature peculiar to each animal species is always in proportion to the quantity of phosphate of lime contained in it.

3. That when the food is found to be deficient in phosphate of lime, the tissues draw from the osseous skeleton that which is necessary for the maintenance of the integrity of the functions of nutrition, as the organism is seen to borrow from the adipose tissue the hydrocarbon elements in which it is deficient in certain cases.

Let us now examine each of these points, and see what consequences may be drawn from them in the study of the subject now before us. And, in the first place, by virtue of what action do albuminised substances assume, in presence of calcareous phosphate, the form of cells and of tissues of every nature, without, however, its being demonstrated that salt of lime forms any constituent part of them?

"Phosphate of lime," says Lehmann, "appears to be a mechanical agent of nutrition." But this term is very vague, and by no means dispels the obscurity that reigns in the mind of the reader. Knowing the property possessed by salts of lime of precipitating albumen in an insoluble state, whenever those two orders of substances are found in presence, might it not be admitted that there takes place in the organism a phenomenon analogous, with this modification—which is, however, quite natural—that in the living medium this precipitate assumes the figured form and becomes organized in tissues?

This interpretation will appear less hazardous if it is borne in mind that, in the muscles, the proteinous substances present themselves in two different physical states: the liquid form preserving the name of albumen, easily assimilable, soluble in water, and containing only alkaline salts; and the solid form under the name of fibres of different kinds, offering a greater resistance to the action of the digestive agents, and containing almost the whole of the calcareous salts.

From this it may easily be understood that if children too young or persons debilitated from different causes are subjected to a course of alimentation exclusively composed of the flesh of young animals (Guérin's experiments), the liquid albumen will be easily absorbed, whilst the solid parts containing the salts of lime will resist the action of the debilitated stomach and will be ejected.

It is, therefore, not organizable elements which are wanting, but an organizing agent, and mineral inanition ensues indirectly as surely as by administering aliments deficient in salts of lime, such, for instance, as wheat alone.

We have said that the vital activity of animals, and their peculiar temperature, are always in proportion to the quantity of phosphate of lime they contain: now, in the physiological experiments that I followed out with M. Dusart, when animals were deprived of phosphate of lime, this privation was attended with a sudden loss of appetite, accompanied with sadness, depression and immobility, as infallibly as the contrary phenomena were produced, in the course of a few days, by administering calcareous salt.

The symptoms thus observed have the greatest affinity with those which mark incipient rachitis, and those periods of adolescence during which, after a rapid stage of growth, the organism, exhausted by the efforts it has just made, and incapable of sustaining it any longer, seems to give way, and reveals a suspension of all the acts of nutrition, and a general atony by which the patient is affected morally as well as physically.

We have then, as in rachitis, pains penetrating the limbs and articulations, complete anorexy, fatigue and difficulty in breathing after the least exercise. This state also reminds us of that of a great number of women during their pregnancy, and of

that of most convalescents when the agents of nutrition are slow in resuming their functions.

To explain the gravity of osseous injuries, we have said that the tissues subjected to mineral inanition draw from the skeleton as from a common reservoir the phosphate that is necessary to them. We justify this assertion by the fact, that in the animals on which we made experiments, we found, at the end of three months, that the bones had become friable and had lost two-thirds of their weight.

Now it is known that in a normal state the osseous tissue requires several years for its renewal; if, therefore, it had undergone only a mere stoppage of nutrition, ejecting the worn-out elements and receiving no fresh ones, it would have lost but a slight part of its substance. Such is not the case, and we do not see that any other explanation could be given of this apparent anomaly.

But how is it that mineral inanition, whether artificially produced in animals or whether it is the consequence of different morbid causes, is manifested by lesions which vary in their characteristics?

A complete answer appears to us to be contained in the consideration of the age of the subjects attacked. If, in fact, vicious alimentation exerts its action on children too young, the invariable presence of ill-digested substances in the intestine will soon produce enteritis and all its consequences; whilst the bones will undergo a shock, the more destructive as they are struck in the midst of the intensest movement of organization. Then follow the thickening of the periosteum, with vascularisation, effusion of blood under the periosteum, and in the bony texture itself.

The latter, mortally injured and struck with a kind of molecular necrosis, is divested at once of its mineral and organic principles, and the phosphates subjected to the destructive influence are abundantly eliminated through the kidneys.

In vain are new tissues formed in the bone to replace those struck with inflammation; they remain in a spongoid or fibrous state till the progress of age or a judicious regimen admits of the introduction of fresh salts, susceptible of organization.

If, on the contrary, the morbid cause attacks subjects having

already attained a somewhat advanced degree of development, if the osseous system, having acquired its normal consistence, now presents only the movement of nutrition necessary for the regular renewal of its different parts, the perturbation will be less, and, though the osseous tissue will be singularly rarefied, we shall discover friability, but no inflammatory phenomena with mollities, except in very rare examples of osteomalacy, mostly occurring in very peculiar cases of late or multiplied pregnancies and under hereditary influence.

In conclusion, let us mention that from the very beginning of the pregnancy, the phosphate of lime disappears from the urines.

The salt is condensed throughout the organism with a view to the future development of the embryo, in the form of a thickening of the bones, and even of concretions, called by Follier osteophytes, which, far from being, as certain authors have believed, the result of an aberration of nutrition, ought, in our opinion, to be considered only as reserves destined to meet the extraordinary consumption caused by the rapid growth of the foetus towards the last months of the pregnancy. In fact, it is at that period that the osteophytes begin to diminish; to disappear completely only when they have supplied the milk, during the first months after the delivery, with the mineral elements with which it is abundantly provided at that period, and which are necessary for the sustenance of the infant.

Finally, if all these perturbations proceed from one and the same cause, solely arising from mineral inanition, to remove them it must only require the introduction into the organism, directly or indirectly, of the salts in which it is deficient.

We are of opinion that the success attending the use of cod-liver oil is owing to no other cause. Let us remark that, to produce such satisfactory results, it is sufficient to administer it only in feeble doses, fifteen grammes a day, for instance, the greater part of which does not pass through the digestive tube, and is to be found again in the stools without having been digested. It will be easily admitted that the oil does not act as food in such circumstances. We must then seek elsewhere the cause of its efficacy. As for us, we believe that it wholly proceeds from the exciting action produced through the whole extent of the digestive tube by the volatile oily acids to which

it owes its well-known odour. Then reappear the regular contractions of the digestive tube, and especially the glandular secretions. This double influence promotes the digestion of the solid parts of food which had, till then, resisted, and carry along with them the salts of lime with which they are impregnated.

The same result is obtained, and our practice for several years past is a sure guarantee of the fact, by presenting directly to the absorption of the stomach phosphate of lime; on the condition, however, that the latter be soluble and require of the economy no labour for its digestion and assimilation.

We lay great stress on this point, for before us, many physicians, without, however, suspecting the general action of phosphate of lime, had made a trial of it on patients with the sole view of obtaining a restoration of the bones; but the constant failure of these attempts had produced such discouragement that all had given it up, to such a degree that, if there are some who in a commonplace way advise the use of it, others formally reject it as useless, whilst a certain number, and among the most modern, never pronounce the name, even relatively to the treatment of fractures and of rachitis.

To discover the reason of this conduct, M. Dusart made on healthy dogs, provided with gastric fistulas, a series of observations on the digestion of the phosphates in use till then. He ascertained that, after long labour of the stomach, an infinitely small part was dissolved by the lactic acid, whilst the rest—that is to say, nearly the whole—passed without being digested, but only disaggregated into an impalpable powder in the intestine, where its most ordinary effect was to produce an obstinate constipation.

It may, therefore, be easily conceived that the phosphate taken into the weakened stomach of persons afflicted with rachitis will undergo no modification, and remain absolutely inert.

Then, imitating the action of the gastric juice on phosphates M. Dusart prepared a body to which he gave the name of Lactophosphate of Lime, containing the salt completely digested and, consequently, capable of being absorbed without requiring any previous labour of the stomach, as he easily ascertained with the dogs already mentioned.

It is this compound that was employed in the comparative

experiments made on animals, and that we have made use of in our clinical researches.

This product is now known to most French physicians by the names of Dusart's Lacto-Phosphate of Lime Syrup and Wine.

The large dose of medicine contained in these preparations—a gramme of Lacto-Phosphate to each tablespoonful of syrup, and to each glass of wine—explains how, under its influence, patients are conscious, in a few days, of a return of appetite and of activity in all the functions of nutrition, forerunners of a permanent cure.

ON THE PHYSIOLOGICAL DYSPEPSIA FOR STARCHY FOOD IN INFANCY.

BY PROSPERO SONSINO, M.D., PISA,

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THE animals of the class of Mammalia at birth find their proper aliment in the milk of the mother, and for a longer or shorter time they feed exclusively upon it. It is remarkable that among the same class of animals there are many which, at a later period, take exclusively aliments from the vegetable kingdom, that is, they are herbivorous. Why this difference of feeding between the first portion of life and the successive stages? No doubt all those animals, when new-born, whilst they find in the milk all the nutritive elements that are necessary for the growth and maintenance of their body, yet are not fitted to digest all the various aliments that offer convenient food to the adult animal. Indeed, if we deprive the new-born animal of the milk, and try to feed it with the usual aliments of the adult, we see very soon that it suffers in its nutrition, and its life cannot be sustained. In the infant, vomiting and diarrhoea manifest themselves, and with convulsions the torch of life is extinguished.

What conditions, then, are those the absence of which in the digestive system of early life renders it unfit to digest all the aliments which are subsequently utilised?

"An interval elapses between birth and the evolution of the first set of teeth; and for this period of life it happens that the sustenance provided for us by nature does not call for any performance of mastication. Thus the production of a liquid support by the female parent for the sustenance of her offspring

during the earliest period of independent existence is in harmony with the edentulous condition of the mouth existing at this time."¹

But we cannot suppose that the impossibility of accomplishing the simple mechanical act of mastication, in absence of teeth, is the only reason of all the difference between the digestive powers of the new-born and that of the adult, as we know that it is not enough to fluidify previously any solid food to render it fit for the alimentation of infants.

To say that the stomach of the new-born and young infant is weaker, and that its function is consequently more languid, is to give an explanation too indefinite to satisfy the physiologist of the present day; an explanation, moreover, which would not be absolutely true, as we know that the milk, a food that needs a very complex process of digestion, finds in the infant all the conditions for its digestion, as well, and *perhaps better*, than in later life.

As the process of digestion consists essentially in the action exerted upon the aliments by different fluids which are poured into the alimentary canal; in order to obtain a good explanation of the question, it is necessary to inquire if there are any differences in the action of digestive juices in early life, compared with the adult age, and then to discover upon what materials of the aliments those differences act effectively.

If we examine the composition of milk, the proper food of the new-born and young infant, which with reason we may retain as the prototype of the complete aliments,² we find in it, besides water and salts, the three organic groups of alimentary principles which concur in different manner to sustain the process of nutrition, viz. the group of albuminoid or *nitrogenised* principles—that of *fatty* principles; and that of hydrocarbonaceous principles. But it is remarkable that the latter are exclusively

¹ Pavy, "A Treatise on the Function of Digestion," &c. First Edition, page 11. London, 1867.

² Prof. Schiff says, "L'aliment complet est celui qui renferme *tous* les éléments de nos tissus." (See *Leçons sur la Physiologie de la Digestion*, par Maurice Schiff, rédigées par le Doct. Emile Levier, tome premier, page 59. Florence, 1868.) And Moleschott calls milk "l'aliment des aliments," or "le prototype des aliments," inasmuch as "il représente à la fois un aliment solide et une boisson; une source de albumine et de graisse, de sucre et de sel." It is not necessary to quote other authors, as on this subject all agree in giving the same value to milk.

represented by a kind of sugar called *lactin*, which, with an insignificant modification, being converted into *glucose*, is rendered diffusible—a substance which could almost be called a true nutriment in the signification given to this word by Corvisart; whereas the starchy principle, which is a conspicuous ingredient of the diet of the adult, and which needs an elaborated process of digestion to be converted into glucose, the material fit for absorption and diffusion, is not at all present in *any* milk. This circumstance of the composition of milk induces us to put the question, “*Is the easiness of digestion of milk in early life to be attributed to its being deprived of starchy principle?*” or, in other words, “*Is it the inaptitude to digest the starchy matter which in early life interferes with the complete digestion of all the aliments which include that material?*”

If we have good reasons to reply affirmatively to this question, then it will follow that in infants there is what we may call a *physiological dyspepsia to starchy matters*, to which ought to be referred much, if not all, the difficulty in the choice of the convenient food for infants.

Before giving an account of some researches in experimental physiology which I have instituted with the view of elucidating this interesting point of infantile hygiene—researches which I communicated, in May last, to the Medical Society of Florence, but which I think may offer matter of interest to the medical world of England—it is convenient to examine what other considerations may confirm this suspicion of the indigestibility of starch in early life.

For this purpose it is of paramount importance to examine the peculiarities of the anatomical conditions of the digestive canal in the infant. It is known, in fact, that zoologists have established some differences in the anatomical characters of the digestive canal, which distinguish herbivorous from carnivorous Mammalia; and that as to man, the anatomical conditions of his digestive canal, being between those of herbivorous and carnivorous, give evidence that he is neither exclusively herbivorous nor exclusively carnivorous, but omnivorous. Now, can we say the same thing as to the early infantile age?

Dr. Routh, who has dwelt much on this question, quoting Burdach, says:—“It is remarkable that suction is the only

faculty for the prehension of food which the child possesses at birth, and even this is soon lost if not practised. The jaws are not so constructed as to permit active movements, nor the gums to bear pressure. The hard palate is but little developed, although the cavity of the mouth is sufficiently wide. There is, moreover, no saliva secreted for the first two months, so that no species of preparatory change can take place in it, as, for example, in the conversion of starchy matters into sugar, through the agency of this fluid (saliva). The mouth is, therefore, merely an organ of transmission and suction. The lips are large, and the tongue and pharynx, uvula, and soft palate are well developed, to secure these ends.¹ The stomach in infants is a small tube-shaped membrane, dilated in the centre, one extremity ending in the œsophagus, and the other in the pylorus, resembling in this character that found in carnivora through life. In position, also, it lies more parallel to the trunk; the large and small curvatures and muscular structures being but very little developed. The liver at birth is unusually large; the pancreas perhaps not more developed than the salivary glands; the intestinal tube is much shorter, and the large intestine approaches more nearly in its length to the small. The cæcum (in which, moreover, it is believed a sort of additional digestion occasionally occurs) is very small. The peristaltic motion is more rapid. All these are evidences that food taken will be kept for a shorter time in the canal, and therefore should be in the condition most favourable for digestion.² Lastly, in no other of the Mammalia is there, in the first periods of life, such a complete absence of teeth. In man they appear latest, and are longest in obtaining their full development. In fine, comparing these appearances with those observed in herbivorous animals, viz. well-developed salivary glands, compound stomachs, sometimes four in number; muscular gizzards, as in some birds; long intestines, large ilium, &c., it is manifest they are the exact opposites to what we find in young infants. As the child grows, the peculiarities which are permanent in herbivorous animals gradually present themselves. The stomach assumes a more horizontal position, the

¹ Burdach, "*Traité de Physiologie*," tome iv. p. 364. Paris, 1839.

² See West's "*Diseases of Children*," pp. 402, 403.

valvulae conniventes become well developed, the peristaltic motion of the intestines becomes slower; in fact, all the changes calculated to retard the food in its progress, and thus to expose it more completely to the solvent juices for digestion, occur. All this proves indubitably that animal, not vegetable, food, is the proper diet for an infant.”¹

But let us see what is the principal difference of composition between animal food and vegetable food. Partly, that in the latter we find more materials refractory to any digestion; that in consequence a more elaborate power of digestion is needed to obtain the nutriment from it. We find in the one and in the other, albuminous principles, fatty principles, saccharaceous principles; whereas *only starchy principles*, which constitute the principal ingredient of vegetable food, are not at all present in animal food, be it meat or any other tissue, or a product of the animal, as milk or eggs.

Therefore, the same physiological and anatomical arrangement that in infants gives evidence of its being not prepared for vegetable food, supports the conclusion that starchy matters cannot be well digested in infancy, and that there is really in infancy what may be called a *physiological dyspepsia for starchy aliments*.

Another valuable confirmation that starchy matters are not well digested by the infant, we have, lastly, in the very interesting observation made by a Frenchman, M. Guillot, who in the autopsy of a number of infants found a jelly-like substance present in the intestines, which to the test of the tincture of iodine gave evidence of containing a large quantity of unaltered starch.²

Having seen how many reasons concur to prove the indigestibility of starch in early infantile life, it is time to speak of the physiological experiments that give support to the thesis which I advocate. But before giving account of my new researches, I must say that some researches upon digestibility of starchy matter in early life have been already made by others, concerning one of the digestive juices, viz. saliva.

¹ Routh's "Infant Feeding and its Influence on Life," &c. Second Edition, p. 75. London, 1863.

² This fact is referred to both by Dr. West in his excellent general remarks on the diet of infants, being a chapter of his valuable work on Diseases of Infancy, &c., and by Dr. Routh at p. 325 of the book already quoted.

Bidder and Schmidt found, on examining the power of saliva to convert starchy matter into glucose, that in some new-born animals that property is defective. The same physiologists collected some saliva of an infant four months and a half old, and found that this fluid converted the glue of starch into glucose with great difficulty and very slowly.¹ Prof. Schiff, in his lectures on the physiology of digestion,² says that the active principle of saliva appears in man only at the age of the first dentition, and confirms all the results obtained by Bidder, who was the first to make researches on this subject.³

Curious is the exception found by Prof. Schiff for a single animal, the guinea-pig, the saliva of which, examined in the first week of life, has well displayed the property of converting starch into glucose. But this exception was not found in any other animal, not even of an analogous kind to the guinea-pig, as the hedgehog.

I insist upon this singularity presented by the guinea-pig, inasmuch as it is coincident with the fact that the same animal at its birth offers conditions of more advanced development than the generality of Mammalia. It is notorious indeed that it is born with open eyes, and very soon is able to run about to search for its food,³ and to render itself independent of the heat of the mother, and that its process of dentition begins very early. Thus the very exception offered by guinea-pigs allows us to conclude that *in the generality of Mammalia saliva acquires its digestive power over starchy matter only at a degree of development which, in the larger number of the same animals, is not reached at the time of birth.*

But saliva is not the only digestive fluid which assists in the digestion of starchy matter. Pancreatic juice and enteric juice also take part in it. As far as I know, no one has made any researches with the view of establishing whether or not these latter juices possess in new-born animals the same digestive power over starchy matter as in the adult animal. The per-

¹ Die Verdauungssäfte und der Stoffwechsel, 1852, p. 23, quoted by Allix, "Etude sur la Physiologie de la première Enfance," p. 121. Paris, 1867.

² See vol. i. p. 206.

³ "Le cochon d'Inde," says Burdach, "broute l'herbe dès le lendemain de sa naissance quoiqu'il tette pendant une quinzaine de jours." See tome iv. p. 439.

formance of such researches on pancreatic and enteric juices is really more difficult than that concerning saliva, inasmuch as these juices are poured into the digestive canal, and we cannot therefore collect them in man as we can do with saliva. Thus the direct examination of the same juices can be instituted only in *animals*. But in the same manner as physiologists have established the general digestive property of pancreatic and enteric juices, especially by experiments on *animals*, it appeared to me feasible to elucidate the function of digestion in early life by analogous experiments on *very young animals*.

In making such researches I had the opportunity of availing myself of the advice and co-operation of my friend, the well-known physiologist, Prof. Schiff, who put at my disposal all the means offered by the laboratory in the Museum of Florence.

Prof. Schiff had long before instituted a large number of researches on the action of pancreatic juice in the digestion of adult animals. We had then to repeat them, as far as possible, in the same manner on young and sucking animals, to give them a value for comparison between the digestion of the young and of the adult.

But to estimate the digestive action of pancreatic juice on starch in very young animals, we could not have recourse to the method of obtaining the pancreatic juice by a pancreatic fistula, as the smallness of the viscus in young animals renders the latter impracticable. We had, therefore, recourse to another expedient, which in the young, as in the adult, could be practicable with the same good result.

The process of experiment is the following:—After having killed the animal, the pancreas is immediately drawn forth, and being isolated, it is hashed and reduced to the consistence of pap. This is put in sufficient distilled water, and thus there is obtained a pancreatic infusion which is endowed with the same digestive powers as the pancreatic juice. If we pour some drops of this *fresh* infusion on some glue of starch, and afterwards apply to the mixture Trommer's test, we shall have evidence whether the transformation of starch into glucose has taken place, and therefore whether the pancreas of young animals possesses or not the digestive action on that material as it does in the adult.

We made the experiments with the pancreatic infusion of five sucking animals, *i.e.* a little dog, five days old; another dog, fourteen days old; a cat, seven days old; and two rabbits, seven days old. The result was, that the fresh pancreatic emulsion of all these animals was incapable of transforming starch into glucose, *even after a long contact*, whereas the same *fresh* pancreatic emulsion of *adult animals* produces the transformation in a very short time indeed, *almost immediately*.

We must add, that if a little transformation began to be detected when the pancreatic infusion was tried a few hours after being prepared, that transformation cannot be attributed to a true digestive action of pancreatic juice, inasmuch as it is well known that many organised matters, and especially those belonging to glandular tissue, upon entering into putrefaction, give rise to a ferment which is capable of transforming starch into glucose as well as pancreatic juice, or saliva, in normal condition can do.

I cannot pass over in silence the fact that researches were made also on *enteric juice*, with the same view of deciding whether the same juice in the young is or is not endowed with the digestive action on starch as in the adult. But I must say that these latter experiments were not so conclusive as the above concerning pancreatic juice. In fact, some of them gave us evidence that enteric juice was already endowed with the digestive action on starch in the same young animals in which we had found the pancreas inactive.

Thus our experiments, if they leave some uncertainty as to the enteric juice, allow us to come to a conclusion concerning at least the pancreatic juice. This conclusion may be expressed in the following words: *Pancreatic juice in dogs, cats, and rabbits in the first week of life—perhaps for some days more—is devoid of any digestive action on starch.*

But can I infer from the above conclusion that the same inability of the pancreas to digest starch in young of the animals above named exists alike in the early life of man?

If the physiologist cannot perhaps accept such an inference as the result of an experimental fact, nevertheless the best founded analogy maintains it, the more so as the animals on which we have experimented are of three different species, two carnivorous and one herbivorous, and as the time of the

sucking life in them is much shorter than in infants. But conceding that my own researches do not afford sufficient arguments to establish the incapacity of the pancreas in infancy to digest starchy matter, and that they have only opened a field to new investigations, to demonstrate a presumed fact that would establish a great difference between the digestion of the infant and of the adult, we cannot, however, forget that the inactivity of saliva has been verified directly in infants. Now this alone, and still more added to the result from M. Guillot's observations above referred to, are sufficient to allow us to establish that *in the early life of man, probably till the beginning of dentition, infants offer a true physiological dyspepsia for starchy aliments, caused by the inactivity of one at least—probably of all—the humours that concur in the digestion of those aliments.*

I think it very important to rigorously establish this condition of *physiological dyspepsia* in infants, which perhaps in the very young reaches absolutely the degree of *apepsia*, inasmuch as the tendency, not only of the public, but also among the generality of practitioners, is too favourable to feed infants with starchy matters. In fact, the preference given to starchy articles of food by mothers when they wish to add something else to the nutriment offered by the milk, or altogether to wean the infant, is notorious; and we find in many countries, at the present day as well as in the past, the common pap, or, what is still more dangerous, rice, arrowroot, or tapioca, which contain a larger quantity of starch than bread, united with the normal aliment of the sucking child; and, still worse, this is usually done more frequently, and with more persistence, when the infants do not thrive and when they are sick, *i.e.* when the digestive power is probably more defective than in healthy infants.

It is very strange indeed that this tendency to feed infants with starchy matters should continue to subsist in our days, after so many writers on infant feeding (among whom several English, as West, Routh, and Eustace Smith, stand pre-eminent) have already hinted at its inconvenience,¹ and

¹ Writing in an English medical paper, I need not cite the *passages* of the above-named writers in which mention is made of this subject. But I cannot refrain from noting that I found very instructive, as full of practical views, both Dr.

long after an eminent physician had given a warning against it. Who is the medical man that has not read, indeed, Zimmermann's words on the danger that arises from the common use of pap in infancy? Zimmermann says:¹ "I know very well that millions of infants are fed with pap, but I know also that it has killed many hundreds of thousands of them. And yet," he adds, "it would be more easy to shake the Alps from their basement than to make a *hare-brained woman* understand the danger that arises from the use of pap."

Yet, with deference to Zimmermann, it is fair to defend the same *hare-brained woman*, when, in giving the pap to her baby, she contrives by simple intuition to render that food less difficult to be digested. I allude to a custom which is very much extended among the countrywomen of Southern Europe. They mash well in their own mouth every bit of pap that is to be afforded to the baby, and thus offer it the aliment mixed with the very juices that contribute to its digestion,—juice which in the mouth of the infant is poured out in small quantity, and perhaps entirely deprived of any digestive action.

However contrary the custom may be to the rules of a refined politeness, we must regard it as a very efficacious manner of facilitating the digestion of starchy matters, and it is one that affords a rough idea of those recent trials of *artificial digestion* which will render great service to alimentation in many cases of dyspepsia, and particularly in infancy.

The trials of administering food previously artificially digested both by gastric² and pancreatic juices, in cases of infantile

West's "General Remarks on the Diet of Infants," &c. in his treatise above quoted, and Dr. Eustace Smith's "Diet of Children in Health and Diseases," being a new chapter added to the second edition of the book on the "Wasting Diseases of Infants and Children." In fairness I must not omit to mention a French author, M. Coutaret, of Rouen, who more recently has dwelt much on the difficulty of digesting starchy matters, in a paper published in *Lyon Médical* of 1870, with the title "Essai sur la Dyspepsie de la Période de la première Enfance." M. Coutaret suggests the use of *maltine*, being a sort of vegetable diastase, for aiding the digestion of starchy food. I have not tried this article, but I confess that I trust as little to it as to the other extractive principles called *pepsine* and *pancreatine*, which I fear much with their preparation must lose much of the efficacy that we find in the artificial digestive juices of which mention will be made in this paper.

¹ "Della Esperienza in Medicina," vol. iii. libro 4, cap. vi. p. 24. Milan, 1815.

² Although the subject of this paper concerns only the dyspepsia for starchy

atrophy, are suggested even in Routh's book already quoted. This author hints at the convenience of mixing with the food the pancreatic juice, particularly for converting starchy materials into glucose; but he adds: "Unfortunately, however, we have not yet accurate knowledge enough to prepare this artificial juice." And then adds: "It is to be hoped, however, the desideratum being known, that some means may be devised by which it may be procured, so as to admit of convenient employment in cases like those under consideration."¹

Now, there is no longer any difficulty in satisfying this desideratum. It is known, in fact, that the infusion of fresh pancreas matters, I cannot refrain from alluding here to Routh's opinion about the advantage that artificial gastric juice may render in being mixed with some foods to render them more digestible in infants. This author suggests mixing artificial gastric juice with the same broth to render it more profitable to the little infantile organism. Now, I must say that recent researches concerning the nutritive property of broth have shown that it contains organic materials which need no digestion for being absorbed, and that it not only constitutes a nutriment already formed, but affords to the blood organic elements which concur in the formation of the organic active principles of the secretion of the gastric and pancreatic juices. Thus, Professor Schiff, to whom we are indebted for this discovery, gives the name of *peptogenes* (see his "Leçons" above quoted, and particularly the 26th) to the broth and to the other aliments, as dextrine, which have this property of affording the necessary elements for the production of those juices. Therefore, it is not necessary to add artificial gastric juice to render broth more fit for nutrition in infancy. That it is so, it suffices to note that there are cases of infants in which there was a complete *apepsia* to every food, including human milk, and that in those cases the sole administration of much concentrated beef-tea, both by the mouth and by the rectum, adding in the enemata some dextrine without any other food, acted like a charm, sustaining the forces of the infant, putting an end to its disorders, as vomiting and diarrhoea; and lastly, serving to restore the digestive power of the stomach and to enable it again to digest milk. For more particulars about the nutritive property of broth, I call the attention of the reader to a valuable paper of my friend Dr. Levier, published in the medical journal the *Imparziale*, in 1869, with the title "Valore terapeutico del brodo." In that paper he refers to an observation, full of interesting particulars, concerning a little baby of a *confrère* that owes the preservation of its life to feeding with broth. It is to be said, after all, that in infants the dyspepsia for certain nitrogenised foods is much more rarely verified. It is, in fact, well known that in some cases of diarrhoea from weaning, the only food that finds conditions to be digested and to restore health is *raw meat*. The use of artificial gastric juice cannot consequently find in infants as frequent indications as pancreatic juice finds for its digestive action on starch.

Notwithstanding the length of this note, I wish to add that in a sojourn in Syria I was assured that, in the Yemen, the Arabs, to bring up better their young colts, give them broth instead of water. If this statement is true, we must infer that in the herbivorous animals, as the horse, broth acts favourably on nutrition as in man.

¹ P. 377 of the work above quoted.

acts like the pancreatic juice itself, and that if we obtain an infusion of pancreas in glycerine instead of water, we have a good manner of preserving it as long. Leube, who recently made some trials of feeding by the rectum, found that glycerine extract of pancreas is quite equal in its digestive power to the fresh pancreas, and will remain good for several weeks. The extract must be prepared with the pancreas of a bullock, which is finely chopped and rubbed with 250 grammes of glycerine.¹ Thus, I think that we must try this new experiment to render digestible starchy matter in infancy.

But now a new question may arise about starchy aliments. Is the danger from them, in infancy, to be referred only to their indigestibility, or also to their affording insufficient materials for the sanguine process of assimilation, and to the feeble infantile process of hematosiis to reduce them conveniently for the ends of nutrition? If the second proposition is true, any expedient contrived with the view of enabling the alimentary canal to absorb starchy materials would be useless, the best thing being in that case to exclude them altogether from the food of infants. On this point it must be recollected that before the indigestibility of starch was hinted at, the danger of feeding the infant with it was attributed to its insufficiency for repairing the waste of the organism, and in this manner were explained all the disorders which arise from that manner of diet, to which was referred also the origin of rickets (Magendie among others).

Good reasons, however, make us now believe that really it is not convenient to feed infants with copious starchy matters, however these may be rendered digestible. These reasons are, that the nutriment furnished by starch does not afford materials for the re-integration of the principal tissues, but it concurs almost exclusively as fuel to the process of hematosiis, whereas the growing infantile organism needs a greater quantity of those nutriments which afford directly materials to the development of the tissues than of the other, which must be consumed in the process of hematosiis. There is, moreover, reason to suspect that this function is in a certain degree defective in the infantile

¹ See the *British Medical Journal* of Aug. 3rd and the *Imparziale* of Aug. 16th. The property of glycerine to preserve unaltered pancreatic juice was discovered by Wittich, of Königsberg, and confirmed by Professor Schiff and Dr. Herzin.

organism, and therefore that it is better accomplished with fatty matters than with starchy matters, the former being more advanced than the latter in their degree of oxidation.

But all these are conjectures, and I think that after all a further experience is necessary to resolve the question more fully.

Now, to conclude, it will be manifest that the object of this paper is principally to prove the indigestibility of starchy food in infancy. I do not pretend, however, to have demonstrated my thesis merely by the physiological researches instituted by myself, as I have also done so by other arguments that belonged already to the patrimony of our science. My researches, I think, have only given a *new direction* to the investigations upon infant diet, showing that physiology may better elucidate this subject by establishing all the differences that exist between the action of the digestive juices upon food in the infant and in the adult. I have only occupied myself with their action upon starchy matters. Thus it remains to investigate the action of the pancreatic juice and of the other juices (gastric and enteric) upon albuminous matters and upon fat, and I wish to call the attention of physiologists to this desideratum, as a study that may throw new light upon a subject of so paramount an importance as infant-feeding.

Reviews.

The Graft Theory of Disease. By DR. JAMES ROSS, Waterfoot, near Manchester. 8vo. pp. 292. London: Churchill.

[SECOND NOTICE.]

IN our last number we gave a *résumé* of the preliminary reasons which induced Dr. Ross to provisionally adopt the theory that zymotic diseases are caused by contagia which consist of living particles—those particles, however, not being independent beings, as many have supposed, but fragments detached from one living organism and implanted upon a second living organism as a graft might be implanted upon a stock. For the purpose of developing this theory, Dr. Ross makes use of the Darwinian hypothesis of pangenesis in a manner which must now be considered.

It will be known by most of our readers that the hypothesis of pangenesis supposes—1. That during all stages of development the cells of the body throw off gemmules, which circulate freely throughout the system. 2. That the gemmules multiply by self-division, and subsequently become developed into cells by union with other gemmules or partially developed cells which precede them in the regular order of growth. 3. That the gemmules are transmitted from the parent to the offspring—are developed in the succeeding generation, but often are dormant during many generations. 4. That the gemmules, in their dormant state, have a mutual affinity for each other, leading to their aggregation either into buds or into sexual elements. We need not dwell now on the boldness and apparent novelty of this much-discussed hypothesis, nor attempt to appreciate any of the criticisms, sensible or foolish, which have been expended upon it. On one point, indeed, we may be permitted to express a surprise that we cannot get over, viz. that the superficial appearance of novelty has not been more generally seen to be, in large part, unreal. As Dr. Ross himself points out, the germ of the idea is perfectly expressed in a passage of Hippocrates' treatise "On Airs, Waters, and Places," and it may be added

that scarcely a man of genius who has handled the subjects of generation and development since the revival of scientific biology, has failed to express, more or less involuntarily, as it were, ideas which are germane to that of pangenesis. Nothing seemed more strange to us than the reported exclamation of one of our foremost biologists (otherwise an upholder of Darwinian views), that "pangenesis was too hard a nut to crack:" since that hypothesis, in so far as it introduces any considerable element of novelty into our speculations, does so largely by the mere statement (in concentrated form) of facts which can readily be verified, though they do not obtrude themselves upon the attention of careless observers. Doubtless it presents the aspect of too great absoluteness, and an unnecessary particularity, but if that were to be treated as a grave objection, it is hard to say what should have become of many hypotheses of the past which have been among the most fruitful for science.

Dr. Ross, however, is no blind acceptor of the doctrine of pangenesis, but while acutely pointing out some of its defects, he applies it in a restricted manner to the solution of his problem. He does not think it necessary, for instance, to assume the presence, in the fertilised germ, of such myriads of gemmules as Mr. Darwin's theory demands; holding, and we think fully proving, that much of the necessary differentiation of parts not only could, but necessarily would, be produced by the influence of the incident forces to which the germ is exposed. But it will be best to present our readers, as soon as may be, with a practical example of the working of Dr. Ross's theory; and the instance which he himself considers most typical—that of small-pox—shall be selected.

Dr. Ross commences the discussion of the "small-pox group" of diseases with the instance of vaccination. How, he asks, is that difference secured between the effects of a common inflammatory pustule and the far profounder influence of the vaccine vesicle (which in other things seems so like it) upon the organism at large? He cannot answer the question directly, but he ranges it alongside of the singular facts, discovered by Darwin, concerning the extraordinary variety of the gall-nuts which are produced by the poison of closely allied species of insects: the variation appearing to depend almost entirely on the matter injected, not on the species of plant which is attacked. Looking at vaccination by the light of pangenesis, we should presume that the vesicle casts off gemmules at the different stages of its progress, which first affect the cells with which they come in contact, the cells becoming less and less affected as the circumference affected widens. But some of those gemmules are absorbed by the lymphatics, and these come in contact with the cells of the nearest glands. The cells are fertilised by union

with the gemmules, become more active, a gentle flow of blood takes place to administer to their demands, and engorgement of these glands results. The vesicle, areola, and glandular irritation are merely facts with which we are familiar from the phenomena of ordinary inflammation, but the special characteristics of the vaccine vesicle imprint a particular character upon the secondary charges,—they are intense, apparently from the fact that the highly active contagious gemmules of the vesicle are able to infect a large tract of tissue at one time, and produce high action in the tissue affected. That the primary vesicle should impress its own character on the disease which it propagates to other tissues, is no more surprising than that a sperm-cell impresses the characteristics of its ancestor upon the individual which results from its union with the germ-cell.

It is not probable, however, that *all* the gemmules cast off from the vaccine vesicle are arrested by the surrounding tissues, or taken up by the lymphatic glands: and Dr. Ross makes use of the phenomena brought to light by what is known as “Bryee’s test” to suggest another destiny for a portion of them. When another part of the body is vaccinated four or five days after the first vaccination, usually the vesicle will appear and go through its changes so rapidly that it will finish its whole course as early as the primary vesicles; and this proves that as early as the fifth after vaccination, the distant tissues have already undergone a modification the nature of which can only be speculatively imagined. Dr. Ross suggests that gemmules are absorbed by the capillaries and act on the tissues, for which they have affinity, two or three days before the second operation. The cells of these tissues will, in that case, have acquired the motion communicated to them by the gemmules, though this motion, if not supplemented, would not be sufficient to cause an external lesion. The lymph of the second vaccination supplies what was lacking for the formation of a vesicle: but this vesicle is both more rapid in its first changes and less intense in its effects than was the primary one: but it *declines* *pari passu* with the earlier vesicle. The suggested explanation of the differences between the history of the two vesicles is, that in the case of the later one the gemmules of the first have already affected the tissues with their motion, and the new impulse being in the same direction, both intensity and rapidity of action are increased, and the vesicle sooner comes to maturity. But the very state of the tissues which results from infection with the gemmules brought from the primary vesicle is one in which they are less dissimilar to the vaccine particles than unaffected tissues would be: hence the second inoculation finds less powerful materials for excitement; for, as in the sperm-cell and the germ-cell, along with close likeness in many ways, there

must be a decided difference between two cells (or sets of cells), in order that any evolution may proceed from their junction.

The evolution of cow-pox is not finished with the history of the vesicle: there is a secondary rash, about the eleventh day, which lasts for about a week; it is not a constant, but a frequent occurrence. Dr. Ross believes this to consist of abortive vaccine vesicles: in the first place because the eleventh day is just the time for the secondary eruption of inoculated small-pox, but also for other reasons. He further thinks it probable that this secondary rash is due to reinfection, by gemmules cast off from the lymphatic glands, of the patches of "adenoid" tissue in the vicinity of the hair-bulbs and cutaneous glands.

The vaccine infection is of course (and Dr. Ross very well sums up the evidence on which this truth rests) only a modification of small-pox itself; and the place which it assumes (in Dr. Ross's graft-theory) is that of an exact reverse of the process of "double-grafting" by which the fertility of certain trees is enhanced. If it be desired to enhance the fertility of a particular sort of pear, and if the common expedient of grafting direct on to the quince is not feasible in the case of that particular pear, then some free-growing species of pear is grafted on to the quince, and the scion so obtained is afterwards grafted with the species of pear whose fertility it is desired to enhance. In this indirect manner the fertilising power of the quince is exerted on the last graft with perfect success. Vaccination represents, according to Dr. Ross, the results of an exactly opposite manner of dealing with small-pox, whereby its fertility is decreased, viz. the passing of the poison through generations of cows.

The above train of thought is a good specimen of the interesting way in which Dr. Ross deals with the central portions of his theme. The whole of the argumentation concerning the genesis and modifications of the more virulent contagious diseases is exceedingly able; and we think that no one who reads it with competent knowledge and understanding can fail to recognise the fact that it is rich in suggestions which will probably be most fruitful. As one might fully expect, however, Dr. Ross has been quite unable to keep within the narrow bounds of an application of the graft-theory merely to the zymotic diseases. That remarkable property of *inflammation*, by which it is always more or less contagious, and hence is virulently so under special circumstances, is dwelt upon with great force, and it seems to us very legitimately: since one of the most pressing questions of the day is the manner in which the phenomena of common inflammation can become intensified and aggravated, until they approach indefinitely near to, or even merge in, the class of fatal infectious diseases. Dr. Ross's line is far more open to question when (in the latter chapters of the

book) he endeavours to strengthen the hypothesis of disease grafting by proving that its explanations apply with equal force to the phenomena of constitutional diseases and of inherited bodily peculiarities, whether morbid or not. We by no means mean to say that under the latter subject he may not possibly, in the end, find even more striking support to the theory of a general bodily transformation produced by influences analogous to the process of grafting. What we do say is, that here he is comparatively weak: that the closeness and cogency of reasoning which distinguish his treatment of the theory of zymotic disease somewhat desert him: and the effect is to make him seem to spread out and weaken his argument unnecessarily. In fact, we can only hope that he will return to the subject fortified and enriched by a larger collection of facts and a generally fuller view of the wider question. We are certainly surprised that of all things he should have omitted or only very slightly dealt with the question of the inheritance of nervous diseases: a branch of inquiry which we suspect he would have found far more fruitful in interesting results, in the direction of his present researches, than is the question of the influence of "scrofula," the inheritance of beauty, and so forth. We must say, too, that we do not think his statements on matters of fact are so careful and reliable in the later as in the earlier chapters: and on the whole we are inclined to regret that he did not limit his application of the graft-theory, at present, to the phenomena of zymotic diseases, and to those of inflammation in its various shades up to intensely contagious peritonitis.

Nevertheless, this little book of 290 pages, to which we have felt obliged to pay the compliment (however imperfectly) of a double notice, must be pronounced to be one of very unusual excellence. It is perhaps regrettable that so distinct a tone of *solidism* as opposed to *humoralism* should pervade it: for, although we believe that the former view of disease is greatly more scientific than is its opposite, yet we fear it is likely to raise prejudice in the minds of many, and in other minds—more sensible—to excite the uncomfortable sensation of a too violent reactionary swing of the pendulum. With all its faults, however, this book marks the entry into the ranks of medical literature of a mind that will leave its impress on the generation—a mind accustomed to reflect on the problems of disease with that force of patient and logical directness and determination which makes precisely the difference between discovery and mere guesswork. We have much satisfaction in assuring our readers that those of them who wish for the unusual pleasure of being obliged to think about a book after they have laid it aside, will find that pleasure richly provided for them in the work of Dr. Ross.

[A number of reviews are unavoidably postponed.]

Clinic of the Month.

Successful Case of immediate Transfusion.—Dr. Aveling, of London, records a case in which sharp hæmorrhage occurred after delivery, in a small, fair lady, aged 21. Mr. Webb, of Maida Vale, who was in attendance, sent for ice, gave ergot and brandy freely, and, grasping the uterus, excited it to action. The hæmorrhage continuing, he found it necessary to detach the remaining half of the placenta, which was unusually adherent to what felt like the partially inverted fundus of the uterus. This he endeavoured to replace, but without success, as he was obliged to cease his efforts at the time lest the shock should entirely extinguish life. Blood continued to flow freely, and the patient became rapidly exhausted and faint, and no pulse could be felt at the wrist. Dr. Aveling was sent for, and found her insensible, with dilated pupils, cold hands and feet, and without pulse in the radial and temporal arteries. The heart's action was weak, and growing perceptibly more feeble. It was evident there was no time to be lost. A fold of skin at the bend of the arm of the patient was raised, transfixed, and divided, when a large flattened blue vein became visible. This was opened, and the afferent tube, with some difficulty, on account of the insufficient light of two candles, was adjusted. The arm of a coachman in the employ of the family was next prepared as in ordinary bleeding, and an incision made directly into the vein sufficiently large to admit the afferent tube. The man was then seated in a chair beside the bed, and the india-rubber portion of the apparatus filled with water having been attached to the tube, the process of transfusion commenced. After a few drachms had been transmitted, Dr. Meadows, who took charge of the afferent tube, thought he felt the skin rising near the incision, and suggested that the tube was not in the vein, but in the cellular tissue beside it. This proved to be true, and the tube had to be taken out and inserted into the vein. Its collapsed condition and the want of light made this no easy task, but it was at length effected, and the transfusion then went on steadily and easily until more than sixty drachms of blood had been injected. As the operation proceeded, the pulse at the wrist

became perceptible, the lips less blanched, and warmth returned to the hands. The patient also became conscious for a short time, and said she was "dying." The mental improvement was comparatively slow, perhaps owing to the quantity of brandy she had taken. In a few hours, however, she became quite conscious, spoke, took nourishment, and began her fresh lease of life. The wound in her arm healed by first intention, but it opened again a few days after, to allow some pus to escape, the result of the accident above alluded to. When the patient was sufficiently recovered she was placed under the influence of chloroform, and the uterus, which had become completely inverted, was returned to its natural position. After this operation rapid recovery ensued, and the patient is now quite well. The apparatus employed consists of a small efferent tube fitted into the vein of the blood-donor, and of an afferent tube fitted into the vein of the patient. There is besides an intermediate piece consisting of an oval elastic bag with a piece of elastic tubing attached to each end, the ends of the tubes being fitted with stopcocks. In the operation the bag is filled with water, and, the afferent cock opened, the water is squeezed into the patient's vein. The afferent cock is closed, and the efferent cock opened, when the expansion of the bag draws the blood from the blood-donor, which is again propelled into the vein of the patient by the compression of the bag, and so on, the known capacity of the bag permitting the absolute quantity injected to be accurately estimated. (*Lancet*, Aug. 3, 1872.)

Disinfection in connection with Small-Pox.—Mr. Charles Tichborne, in a paper read before the Medical Society of the College of Physicians, states that on the first week in November, 1871, in a large establishment, a case of small-pox occurred. As there were about two hundred beds in this establishment, considerable alarm was felt by those with whom rested the responsibility of management. A consultation was held, and a well-known and well-advertised disinfectant was used with every precaution as regards cleanliness. The consternation was great when case after case was sent out of the house until the eleventh was taken to the hospital on the 6th of January, who died on the 11th of the same month,—a case every fifth day. A consultation was again held, and with advice the following plan was adopted:—The disinfectant was changed, and the use of carbolic acid and chlorinated lime was agreed upon. The carbolic acid (pure) was chiefly used in water-jugs, a few drops in each jug. A man was told off especially to disinfect the place, and to do nothing else. In the morning he made his solution of "chloride of lime," about $\frac{1}{2}$ lb. or 1 lb. to the gallon of water, in a large tub. It was allowed to subside, so that it

was quite bright and clear when he wanted it for use. At three o'clock P.M. he went through all the rooms, sprinkling the solution over every floor, and the windows were left open, so that the rooms were dry by the time they were required for use, equal attention being paid to the mechanical cleaning of the walls. The change in the system of disinfection was made on the morning of the 11th of January, the day when the last case was buried, and from that day forward there was not a single case of small-pox in this seething mass of humanity. Permanganate of potash is invaluable for certain special applications. (*Medical Press and Circular*, July 31, 1872.)

Treatment of Uterine Tetanus.—Dr. Alexander Milne, of Edinburgh—after referring to the extreme difficulty of performing podalic version, where the liquor amnii is absent or has long escaped, where the parts have become swollen and dry, and the uterus has contracted firmly around the foetus—describes the means which he has found most beneficial in enabling version to be effected. In the first place, he considers chloroform should be administered, and very freely too; in many instances it relaxes the spasm in a wonderful manner, but he has seen it fail frequently in uterine tetanus. In the next place, when chloroform is ineffectual, recourse should be had to tartarised antimony and lobelia inflata. A grain of the former may be given and twenty-five minims of the tincture of lobelia inflata; after the lapse of twenty minutes the patient should be placed under the influence of chloroform. The triad form a potent antispasmodic whose conjoint action is almost irresistible, and the spasm is overcome. The uterus being relaxed, the hand finds entrance, and version becomes a possible thing. In regard to the hand to be employed, which he considers to be a matter of capital importance, he holds that the left in the great majority of cases is the proper one to employ. It is the best in all dorso-anterior cases, and better success is obtained with it than with the right, even in dorso-posterior ones, where there is fluid and space. Lastly, in reference to the point whether one leg or both should be seized, Dr. Milne observes that “in difficult cases, to speak paradoxically, we can neither afford to seize both nor to want both—that is to say, there is little room to enable us to get hold of both, but it is better to have both if at all practicable. The foetus will then revolve more readily and come down more speedily.” Dr. Milne records a series of cases which support his views. (*Lancet*, August 10, 1872.)

Treatment of Urethral Fever.—Dr. Brinton, of Philadelphia, discusses at length the subject of urethral fevers, which he says may follow any of the ordinary operations upon the urethra, no matter how carefully or how delicately performed.

The symptoms are, first, the *chill*, which usually makes its appearance six or eight hours after the time of operation, and lasts for from twenty to sixty minutes. It is almost always accompanied by considerable nausea. The pulse is quick and feeble, commonly ranging from 110 to 130, and sometimes wanting in regularity. To the chill succeeds the hot *fever*. This rarely lasts longer than one or two hours, and is again followed by the *sweat*. The patient is literally bathed in perspiration, and so remains for many hours. Dr. Brinton has seen the sweat last almost without break for two or three days, and it is accompanied by very great gastric irritability. The pulse is quick, but it is not strong; the urine is scanty and high coloured; the tongue is coated at first with a white fur, which gradually becomes almost of a chocolate brown, and very dry; thirst is extreme, and there is insomnia. Occasionally such patients die; usually, however, they recover. In regard to treatment, Dr. Brinton first asks whether there are any measures which may be resorted to, to lessen the likelihood of the occurrence of all these disagreeable symptoms. He himself takes unusual precautions, giving the patient some whisky or brandy before operating, and taking care that during the operation he is not in any way exposed to cold or draughts of air. Then, after the operation, he generally empties the patient's bladder with a catheter, and directs that he be wrapped in a blanket and carried to bed, all wet clothes being removed. As soon as he recovers from the anæsthesia, he administers (as has been recommended by Sir Henry Thompson) a full dose of morphia and quinia—one-quarter of a grain of the former and five grains of the latter, and every two or three hours afterwards, until he sleeps, one-eighth of a grain of morphia and two or three grains of quinia, or more, if necessary. He seldom leaves any instrument in the urethra, as they often, he thinks, produce irritation. Soothing applications to the supra-pubic region and to the perineum in the form of hot cataplasms, dashed with laudanum, or the old-fashioned excellent hop poultice, are very serviceable. If, in spite of these precautions, a chill should occur, hot stimuli should be given and hot-water bottles and dry warmth be applied. The sweat must be treated with morphia and quinia. The muriated tincture of iron would seem at first sight to be peculiarly adapted to the sweat, but its tendency to increase rather than to allay the gastric irritability renders it objectionable. As a local application, Dr. Brinton recommends frequent sponging of the entire surface of the body with strong tepid alum water, which proves extremely grateful to the patient and appears to check the excessive diaphoresis. The nausea and vomiting can best be controlled by the internal application of a few drops of chloroform and

by the application of mustard plasters upon the pit of the stomach. The diet is a matter of great importance, and Dr. Brinton advises that reliance should at first be placed exclusively upon iced milk and milk punch, given in small quantities; beef essence may then be given, and subsequently solid food. The thirst is best allayed by iced lemonade. Instruments should not again be passed till all irritation has passed away. (*Philadelphia Medical Times*, 1872, p. 241.)

M. Ollier's Occlusive Dressing.—In the last two numbers of the *Lyon Médical*, M. Poncet brings forward some new illustrations of the great advantages attending the employment of M. Alphonse Guérin's mode of dressing wounds with cotton, and associating, as recommended by M. Ollier, with the silicate bandage, so as to constitute what the latter surgeon terms *occlusion immobile*. The object of these papers is, however, not to set forth the general advantages of this plan, which have now been made pretty widely known, but to show its great utility in warding off those hospital complications which have so often excited terrible effects—pyæmia, erysipelas, and hospital gangrene. During the last half-year more than 100 of the *bandages ovatés-silicates* have been applied in M. Ollier's ward (containing 100 patients) to every wounded surface, and not one of these patients has succumbed to pyæmia or been attacked by erysipelas. The bandage in these cases has been kept on for a week or a fortnight, and sometimes for three or more weeks; all accidents from such retention being guarded against by paying attention to the pulse, temperature, and feelings of the patient. The bandage has had to be removed wholly or in part in a few cases, on account of the imperfect way in which it had been applied. In all cases, wherever possible, such removal has not been performed in the wards where the other patients were; the wound being also kept exposed for the shortest possible time; and the quite new, fine cotton taken only from the parcel as wanted. The portion of this brought in immediate contact with the wound has usually been impregnated with carbolic acid or alcohol. In this mode of dressing the minutest details must be considered, and the great object of exclusion of air can only be attained by the superposition of several thick layers of cotton, which must reach very far beyond the surface of the wound. Thus, after an amputation of the leg, it does not suffice to cover the stump with the silicated bandage; but this must also comprise the thigh and the trunk. Without such precautions it becomes a bad mode of dressing, retaining pus which has undergone change from action of the air in contact with the wound. Before the bandage is applied, as many arteries as possible should be secured, and for this

purpose the wound should be left exposed for fifteen to twenty minutes.

M. Poncet gives various details concerning the chief classes of wounds which were treated during this period; and he observes that, if these cases were to be taken as conclusive, this procedure might be pronounced as an absolute preservative against these complications. No case of pyæmia, erysipelas, or hospital gangrene was observed in cases so protected, although the last two affections prevailed in the ward. However, he wishes no exaggeration upon the subject, and observes that while failure will occur wherever the bandage is imperfectly applied, even when its adaptation is perfect, we only place the wound in the very best condition for healing by preventing complications from without. Its healing, however, of course depends also upon other circumstances; it is a means especially adapted for hospital practice, and is not required in cases treated in localities where the air is not likely to be contaminated. The cases M. Poncet relates were also treated in winter, but in summer the dressing may have to be removed every five or six, instead of every fifteen or twenty days. (*Medical Times and Gazette*, August 3rd, 1872.)

Extracts from British and Foreign Journals.

Hydropathic Treatment of Typhus at Bremen.—A report furnished by Dr. Scholz, the director of the establishment, states that since the year 1868, 125 cases of abdominal typhus have been hydropathically treated in the Bremen Infirmary, of which 82 were men and the remainder females. The mortality was scarcely four per cent. The plan adopted was immersion in water at a temperature of from 10° to 20° Cent. (50° to 68° Fahr.). The temperature was taken every three hours, or in bad cases every two hours: in the axilla a temperature of 39° C. (102° F.) was considered as an indication for a bath. When immersed the patient either moved himself or was lightly rubbed. The duration of the bath was from ten to fifteen minutes, or in many cases not more than five or six minutes. He was placed in bed without being dried, and was covered with a woollen coverlet. When the brain was much affected, cold douches were directed against the head whilst the patient was in the bath, and ice-caps were occasionally used. Cloths dipped in cold water were placed at intervals of a quarter or half an hour upon the chest and belly. These appear to exert a favourable influence upon the diarrhoea, and were felt to be agreeable to the patient. Scarcely any drugs were administered. The diet was light but nutritive. Scholz regards the withdrawal of heat as the main object of the cold baths: he does not attribute much value to them as agents for producing a better distribution of the blood, or as acting upon the nervous system, or improving the function of the skin. Hence, in very severe cases where there is no material augmentation of the animal heat, he does not think it right to use them, but brings the skin into action by powerful thermic and mechanical irritation (frictions). The general results of the reduction of the mean daily temperature in typhus caused by the baths are that the course of the disease is milder, the type of the disease more simple, and the cure more certain and expeditious. In four per cent. of the cases, intestinal hæmorrhage occurred, for which full doses of the perchloride of iron were administered. Among the secondary diseases that were observed were peritonitis from perforation, two

cases of articular rheumatism, and some cases of erysipelas and abscess. In two fatal cases, ulcers of the larynx were observed. In convalescence from severe cases temporary hyperæsthesia of the toes was observed. Relapses occurred in four per cent. of the cases. Scholz distinguishes two periods in the treatment of the disease—in the first or early period the patient must not be bathed at night, and not more than five times during the day: whilst in the second or later period the number of baths may be sufficient to keep down the temperature of the body to near its normal degree. The same plan of treatment was tried and found very advantageous in scarlet fever, measles, diphtheria, and erysipelas. (*Rundschau*, June 1872.)

Hypodermic Injection of Quinine.—The salts of quinine, and especially the hydrochlorate, says Dr. Otto, are not sufficiently soluble to be commonly employed in subcutaneous injections, and the surgeon can neither be sure of the dose required, nor of the rapidity of their action. He recommends the use of pure quinine dissolved in ether; this solution is much less irritating than either the acid or alcoholic solutions. Quinine dissolves in ether in sufficient quantity to produce a prompt action, and to permit a considerable dose to be injected. The quinine should be dissolved in the ether, which should then be filtered and allowed to evaporate to some extent, so that a more concentrated solution may be obtained. The solution he uses contains, in about half a drachm, five grains of sulphate of quinine. Dr. Otto has never observed any local inflammation caused by the injection of this solution, and he has injected as much as five grains of the quinine at one time without finding any other inconveniences than those which ordinarily accompany large doses of quinine, such as buzzing in the ears. The injection of this quantity rapidly produces a depression of the temperature of the body amounting to 1° C. Hypodermic injections of quinine are particularly suitable to cases of puerperal fever, and those of purulent infection; but they may also be employed with advantage in cases of intermittent fever. (*Le Mouvement Médical*, June 1872.)

Application of Powdered Camphor in Hospital Gangrene.—M. Netter states that he was called in consultation to a case of trauma, in which the patient was attacked with this grave complication, and in which the surgeon in attendance feared the result, notwithstanding the employment of the ordinary means of perchloride of iron, carbolic acid, &c. The appearance of the wound called to mind the phagedænic syphilitic ulcer. But in this last form a particular remedy succeeds admirably, namely, the free application of powdered camphor,

that he had hitherto employed empirically and against this affection alone. This was tried in the case just mentioned, and with perfect success. A second case attended by fortunate results has been recorded by a well-known naturalist, M. le Vaillant, who had care of the wounded in the Hospital of St. Mals. In a third case, which proved equally successful, M. Netter noted a peculiarity which may perhaps afford an explanation of the mode in which camphor effects a cure. The dry matter of hospital gangrene liquefies in contact with camphor, in virtue, no doubt, of the well-known action of camphor on the fats. Hence camphorated ointments must be kept in dark places, whilst pure lard is unaffected by exposure to ordinary conditions. The camphor may also perhaps destroy the peculiar ferment. Before applying the camphor to wounds affected with hospital gangrene, they should be lightly syringed with water. (*Comptes rendus*, tome lxxii. p. 216.)

The Rate of Growth of the Nails as a means of diagnosing certain Forms of Paralysis.—Dr. S. Weir Mitchell states, as the result of his observations, that the growth of nails is usually retarded more or less in all palsied limbs, whether the paralyzing cause be spinal, cerebral, or belonging to a nerve-trunk. As yet, owing to want of opportunity, he does not know whether or not there is any complete temporary arrest of growth in spinal or peripheral disease and injury. As regards the latter, he is disposed to believe that there is not an arrest, but only more or less retarding of growth and deformation.

In cerebral palsies, whether from clot or embolus, there is an entire cessation of nail-growth on the palsied side. Usually, when they begin to grow again it is a sign that the power of movement will also improve within a few days. The rate of growth slowly increases, but it generally requires four or five months for such nails to produce an entire length from matrix to free edge. To study the change, Dr. Mitchell stains the nails of both sides with nitrate of silver or nitric acid; the latter is preferable, because it soaks into and stains of a deepening yellow the whole thickness of the nail. Staining is not, however, essential except for comparison, because the line of arrested growth is marked by a deep groove, which for months may be seen as it passes down the nail, so that when accustomed to the rate of growth the place of this furrow will enable an observer to guess pretty well at the date of the attack of paralysis. The palsy need not be complete to cause this arrest. It is found in cases involving either cerebral motor palsy or sensori-motor paralysis, but as yet he is not aware whether or not in the rare cases of pure sensorial palsies of cerebral origin it also exists; nor as yet has he any experience which enables him to say

whether or not in sudden spinal palsies there is also complete cessation of nail-growth.

These observations have naturally led him to a close study of the nutritive changes as regards growth and repair of hair and skin in the cases alluded to, but as yet he is hardly ready to speak with confidence upon subjects such as these, which promise to open a rich field in differential diagnosis. It seems possible that the nail-growth may not be altered in the same degree by lesions of the cerebrum, cerebellum, pons, and corpus striatum; and he has some observations which appear to point hopefully to these facts of nail-growth as a future means of aiding us to tell what parts of the brain have been attacked.

Very recently, one distinct and, as Dr. Mitchell believes, most valuable practical contribution to diagnosis has come out of his observations. It is briefly this:

In all sudden cerebral palsies, the nails cease to grow. In hysterical palsies of one limb, or both, whether paraplegic or hemiplegic, the rate of nail-growth is unaltered. (*Philadelphia Medical Times*, June 1, 1872.)

On the Detachment of Urinary Concretions from the Walls of the Bladder.—Dr. Reliquet contributes several papers on this subject to the *Wiener Med. Zeitung* for May and June, and arrives at the following conclusions:—1. That electricity rapidly occasions a diminution of the sensitiveness of the bladder, causing a swift extension of its walls, which leads to a separation or detachment of such urinary concretions as adhere to the walls by a large surface. 2. The continuous irrigation with simple water or with various solutions is capable of loosening and altogether detaching the sand which is fixed in the vesical walls by its rough and pointed surface. (*Wiener Med. Zeitung*, May 1872.)

Albuminuria and Diseases of the Urinary Organs during the Period of Fertility in the Woman.—Dr. R. Kaltenbach draws the following conclusions from his long experience in the diseases of women. Albuminuria does not exist as a physiological symptom either in pregnancy or during the puerperal state. During pregnancy albuminuria may occur: 1. In consequence of vesical catarrh. 2. In consequence of general catarrh of the urinary passages, with marked pyelitis. 3. In consequence of stasis in the renal vessels. 4. In consequence of parenchymatous diseases of the kidneys. In the puerperal state it occurs: 1. In vesical catarrh, which may be called forth (*a*) by chemical or mechanical irritation, as by the use of the catheter; (*b*) by extension of inflammatory processes from the sexual organs to the bladder; (*c*) by severe labour. 2. In general catarrh of the bladder with pyelitis. This originates (*a*) in con-

sequence of simple extension of primary vesical catarrh, (*b*) from contiguity with neighbouring parts in a state of inflammation, especially the inner surface of the urinary organs or the outer side of the parametrium. Pyelitis sometimes occurs as a perfectly independent idiopathic disease. 3. In stases occurring in the renal vessels in consequence of uncompensated valvular disease, or in consequence of large strumous swellings. 4. In tissue diseases of the kidney, (*a*) as a primary affection (nephritis parenchymatosa); (*b*) as a secondary affection, (*a*) as an extension of inflammatory processes from the bladder into the urinary passages and urinary tubuli, (*β*) mechanically, by stoppage of the urine owing to compression of the ureters in the parametrium; (*c*) as metastatic inflammation in pyæmia. It is obvious that several of these causes of albuminuria may exist. (*Med.-chir. Rundschau*, June 1872; and *Archiv f. Gynecol.*, Band iv. Heft 1.)

Nitro-muriatic Acid as an Hepatic Stimulant. — Dr. Kidder, of the United States Navy, observes that the use of nitro-muriatic acid as a liver stimulant is no new thing, having often been tried and found wanting in various cases of emergency, but this he agrees with Dr. Martin in thinking is due, not to the remedy, but to the mode of its application. The acid should always be made extemporaneously by adding to five parts by volume of strong hydrochloric acid, three parts of nitric. The additions must be made gradually, shaking the bottle well each time. After the acids have been well mixed the bottle or jar should be left unstopped for twenty-four hours before use. Three fluid drachms of this acid diluted with one pint of water form a lotion which is to be applied over the region of the liver twice a day *with brisk friction*, while at the same time the feet are immersed in a bath of the same. The hands of the attendants making the application should be protected by oiled silk gloves, or bilious diarrhoea will result from absorption of the acid through the palms of the hands. If the practitioner please, he may give acid, internally, in the usual doses, but its administration in this method is not effective, and is very likely to disturb the already vitiated processes of digestion. The theory of this method of medication appears to be that, since the usual road of remedies, the portal vein, is effectually blocked up by disease, recourse must be had to absorption into the general circulation, with the expectation of reaching the liver through the medium of the hepatic artery, its proper source of nutrition. Applied in this manner, both Dr. Martin and Dr. Kidder maintain that the obstruction to the portal circulation will be so far removed as to prevent entirely the recurrence of ascites, and to secure to the patient a degree of health and comfort not otherwise to be obtained. Dr. Kidder records the particulars of three cases, in

two of which considerable improvement appears to have taken place. (*The Western Lancet*, No. 5, 1872).

Vehicle for the Internal Administration of Chloroform.

—Dr. G. W. Murdonck, having tried various formulæ proposed to facilitate the injection of chloroform, has found that some were difficult of execution, others contained sulphuric ether, and others again contained but little chloroform. He considers the best proceeding to consist in dissolving the chloroform in glycerine (1 : 3), which is effected with tolerable facility, and gives a very clear solution, pleasant to the taste, and with a strong odour of chloroform. This solution can be mixed in all proportions with water without the occurrence of any precipitation, though the odour is distinctly perceptible. In forming the mixture it is well to add the chloroform slowly, and to mingle the two thoroughly. It should be left at rest for twenty-four hours; at the expiration of this period a portion of the chloroform will be found to have collected at the bottom of the vase; this should be separated and mixed with an additional part of glycerine, when no further separation will occur. This mixture may be kept for some time without any loss of chloroform by evaporation. (*Journal de Pharm. et de Chimie*, 1872.)

Fluor Albus, or Catarrh of the Female Genital Organs.

—In a paper on this subject by Prof. Hildebrandt of Königsberg he remarks that simple catarrh of the vagina is of rare occurrence; the seat of the affection is much more frequently to be sought in the uterus and its appendages, but often also in the commencement of the genital canal, the vestibule. In forming the diagnosis the following points have to be attended to. 1. The microscopical examination of the secretion. When there are albuminous tenacious glutinous masses with numerous mucous corpuscles, the cervix is affected; the appearance of large quantities of ciliated epithelium indicates disease of the corpus uteri, and perhaps of the Fallopian tubes. When columnar epithelial cells are in great abundance, there has been protracted catarrh of the mucous membrane of the cervix. The microscopical elements of vaginal catarrh consist of cloudy glutinous serum with pavement epithelium, infusoria (*Trichomonas vulvæ*, elongated bodies with two whip-like processes) and fungi (*Leptothrix vaginalis* and *Oidium albicans*). 2. The chemical evidence. A secretion, which reacts strongly alkaline, belongs to the cervix: the pure secretion of the vagina is always acid, the secretion of the corpus uteri is neutral.

3. Ocular inspection. Amongst the various forms of specula the tubular white glass (milch glas) speculum of Karl Mayer, and the silver speculum of Ferguson with obliquely cut ends, are the best. Sims's, he thinks, on account of requiring two assistants,

are indelicate and cause pain. In most instances the origin of the fluor albus is the portio vaginalis. In virgins the vaginal portion will be found elongated, swollen in the upper parts, and of a dark red colour; around the os, there are circular patches when the epithelium is lost and where ulcers have formed, and from it a plug of mucus projects. In women who have had children the vaginal portion is cylindrical and dark red; the broad, somewhat everted lips are eroded, the finger introduced into the cervical canal finds the mucous membrane swollen with knobby granular elevations consisting of the hypertrophied glands and papillæ. The consequences of catarrh of the cervix vary in the virgin or childless woman and the multiparous female. In the former the quantity of blood lost in menstruation is increased in consequence of the congestion present: intercurrent hæmorrhage also occurs. The external orifice becomes contracted, and the stagnating secretion of the cervical glands distends the canal of the cervix, and at a later period the cavity of the uterus itself. From time to time the secretion is discharged from the thin-walled and flaccid uterus with pains resembling those of labour. The uterus yields to the pressure of the superincumbent viscera and falls backwards: so that a retroflexio uteri is not an uncommon sequence of uterine catarrh. Such women are almost invariably childless; since through the swelling of the plica palmata and the bending of the uterus the transmission of the spermatic fluid is arrested. Other associated secondary symptoms are, cardialgia; loss of appetite, vomiting like that of pregnancy, which last is not unfrequently dependent upon cervical disease, and especially ulceration; irritation of the bladder, with strangury and neuralgia of the uterine nerves, due to friction of the sore os uteri against the posterior walls of the vagina, on which account the patients avoid taking exercise and recline much upon the sofa. A very large proportion of obstinate cases of chlorosis and hysteria owe their origin to neglected catarrh of the cervix. A different series of phenomena occur in women who have borne children, and in whom the cervical canal is dilated and the passage free. The constitution here suffers only in consequence of, and in proportion to, the amount of albumen eliminated. Ulcerations of the os, when the catarrh is of long duration, are constant, appearing first on the anterior lip, as the longer one, and as most exposed to the discharge of the alkaline secretion, and sooner or later little tumours occur upon these ulcerated surfaces. These sometimes consist of glands imbedded in the mucous membrane, which hypertrophy and grow into long-stalked mucous polypi containing cyst-like cavities and varying in size from a bean to a cherry. Sometimes the tumours are composed of hypertrophied interglandular connective tissue, forming broad-based swellings. These are in-

jurious to the patient only in consequence of the constant serous discharge from the cervix and the irregular menstruation they occasion. The so-called papillary form of growth is of a much more serious nature, not unfrequently degenerating into cancrroid.

In most cases cervical catarrh is established during pregnancy or delivery; which even when quite regular is accompanied by a certain degree of it, especially during the last few weeks. The superficial erosions that then occur usually heal up during the puerperal state; but, when there is a want of cleanliness, or compulsory exercise is taken, as at the sewing machine, or when there is a foetid discharge from the uterus, the slight ulcerations extend and deepen. Masturbation, and the use of the sewing machine with treadles, produce an amount of friction of the labia and clitoris that leads to the catarrh. Constitutional affections may also induce it; amongst these scrofula and tuberculosis are most common. In a future number the appropriate treatment will be considered. (*Der practische Arzt*, April 1872.)

Respiratory Murmurs.—Dr. James R. Leaming states that the subject of the respiratory murmurs has occupied a large share of his attention during the last ten years, and he has been led to form some novel views upon their nature. The ear accustomed to auscultation, he says, after a few moments of concentration of the attention upon the respiratory murmur will recognise its dual composition; the *tidal air sound* will be heard in inspiration only, soft and short, like breathing gently through the closed teeth, increasing in fulness in inspiration and diminishing in expiration. It is of low pitch and is like the roaring of the sea at a distance, the waves breaking on an even shore of sand, or, better still, like the sound made by bees in cold weather when the hive is tapped with the finger. If the breath *be held*, this murmur may be heard without admixture, for there can then be no bronchial murmur. The sound is the susurrus of the delicate muscular fibres of the true respiratory system contracting and relaxing over the dilating and resisting residual air. If the breath be held after a full *inspiration*, the murmur will be at its maximum, if it be held after *expiration* it will be at its minimum, fulness. It cannot be exaggerated, as has been said of the so-called vesicular murmur. If the true respiratory system be unduly dilated, it loses its power to contract on the residual air, and the murmur wholly ceases. This is a sign of emphysema, and is proof of the muscular cause or origin of the sound, which may return again after rest. This murmur only commences to be developed in the child at eight years of age, becomes recognisable at twelve, but is only fully developed at maturity. A beginner in auscultation may recognise true respi-

ratory murmur in a good subject, with ease; but when the chest has lost its excellent quality as an acoustic chamber by physical changes resulting from inflammation, or when, from disease of the lung itself, the natural respiratory murmur has been altered or lost, or when the chest, although in its natural conditions, may be covered by thick and hardened muscles, the trained, expert ear only can arrive at diagnostic truth. Besides this persistent sound there is a second or broncho-respiratory murmur. This may be studied by forcing the breathing, when it will be heard both in respiration and expiration; and its harshness, loudness, and pitch will depend upon the force given to the respiration. It may be heard in perfection in the chest of a child, before the true respiratory murmur has been developed. If the chest of a person in health be examined, and the existence of the true respiratory murmur in its fulness and perfection be established, it will be found, if he be again examined after suffering from a chill, with pain in the back, head, and limbs, &c., that a change has taken place; the true respiratory murmur may still be heard, but it will be obscured or muffled. All the capillaries of the lung are gorged with blood, and this is the explanation of the muffled murmur. If the disease progress, in a few hours the true respiratory murmur is no longer audible, but in place of it is a fine crepitant râle. The congestion of the capillaries of the lungs still remains; there is scarcely a perceptible difference in the percussion note; the residual air still occupies its seat in the true respiratory system, and it still continues to dilate the air-sacs, alveoli, and terminal tubes. Whatever change has taken place must have been at the seat of the true respiratory murmur. The change consists in thickening of the connective tissue of the lungs with plastic material. Thus thickened and stiffened, it is unable to contract, and the true respiratory murmur is gone; but it must yield, though unwillingly, to the dilating force of the residual air, increased one-tenth at each inspiration, separating newly formed plastic exudations, causing sound which we hear as a pin crackling, and call it crepitant râle. In a few hours more the crepitant râle as well as true respiratory murmur have gone and all is silent, or there may be bronchial or tubular breathing. Exudation has been poured into the true respiratory system, and consolidation is the result. The seat of crepitant râle is now become the seat of exudation. (*New York Medical Journal*, No. 5, 1872.)

Hypodermic Injections.—In an article contained in the *Bulletin Thérapeutique*, translated in the *Pharmaceutical Journal*, M. Adrian observes that in the preparation of a solution for hypodermic injection, the choice between the normal alkaloid and its salts is not an indifferent one. When medicaments so

powerfully active are injected under the skin, it appears necessary to define clearly the relation that exists between the alkaloid itself and its combinations with acids. In medical practice sufficient importance is not always attached to this distinction, and instances are sometimes met with where the same doses of hydrochlorate, sulphate, and acetate of morphia, or of hydrochlorate and sulphate of strychnia are prescribed, although the constitution of these various salts assigns to them a sensibly different proportion of the active principle that is employed. This will be seen by an examination of the following numbers.

	Crystallised Alkaloid.	Water necessary for solution.
One gramme (15½ grains) of hydrochlorate		
of morphia contains	0·80	20
Sulphate of morphia	0·76	10
Acetate of morphia	0·86	5
Sulphate of strychnia	0·75	10
Hydrochlorate of strychnia	0·83	8

According to this table, one gramme of acetate of morphia contains a tenth more of morphia than the same weight of the sulphate, and a similar difference exists between the hydrochlorate and sulphate of strychnia. Certain difficulties also are met with in the preparation of the solutions, the solubility of the same salt, probably owing to its state of hydration, varying at different times. Filtration is also requisite in most instances, and the filter absorbs an unknown quantity in one experiment; no less than one-fifth of the total amount of the salt being thus retained. Solutions intended for hypodermic injection, prepared as they usually are, present another defect, namely, the alteration which they undergo after a time. Small fungi are seen to form upon their surface; then the liquid becomes turbid, and gives rise to a copious deposit. M. Adrian's experiments have shown him that the solutions of atropia and codeia are decomposed more rapidly than the others. Also when they are prepared in the cold they change more rapidly than when they are obtained with boiling distilled water. Liquids containing the alkaloids dissolved with the help of sulphuric acid are preserved better than those in which hydrochloric acid is used. Finally, solutions containing glycerine to the extent of one-fifth of the total volume may be kept for a long time without undergoing the least alteration. Based upon these observations, M. Adrian has been led to consider the following conditions to be desirable in the preparation of solutions for hypodermic injections:—1. To use exclusively alkaloids of vegetable origin in a state of purity. These are always well defined, stable, and uniform in composition, whilst

their salts vary according to the equivalent of the acid which is used in their formation; and according also as they contain more or less water of crystallization. 2. To use as a vehicle boiled distilled water containing 20 per cent. of glycerine. 3. To give the preference to sulphuric acid diluted in the proportion of one of acid to ten of water, above all other acids. 4. To substitute measurement by volume for measurement by weight. (*Pharmaceutical Journal*, July 6th.)

Arsenic as a Prophylactic in Rabies.—Dr. Ernest Guisan, in an inaugural dissertation presented to the faculty of Berne, states that though he has arrived at no positive conclusion, he believes that, as in cholera, the germ of the contagion in rabies is formed by one of the lowest fungi. The period of incubation extends, upon the average, over five or six weeks. The poison is then absorbed, spreads itself through the body by means of the circulation, and then multiplies indefinitely, producing ultimately irritation of the nervous centres, and especially of the medulla oblongata. Dr. Guisan then enters into the prophylactic treatment of the disease by means of arsenic, and gives the following clinical observations: "A man was bitten on the 24th of June by a mad dog, in the hand; a girl was bitten at the same time, and shortly after died from hydrophobia. Two days after the accident the man applied to Dr. Guisan's father, who cauterised the wound deeply with potash, and kept the wound open with cantharides. Minute doses of belladonna were given morning and evening up to the 18th July, when the patient had rigors and pains in the body. From this time, up to the 26th of July, the symptoms of hydrophobia became gradually more and more expressed in spite of repeated venesections and the use of calomel and opium. At this date, however, small doses of arseniate of soda were prescribed every four hours (0.003 of a gramme). On the 27th, marked amelioration of the symptoms was observed, which continued till, on the 30th, all danger had passed and complete recovery took place." Dr. Guisan gives another case in which a rabid dog, between the 7th and 9th of June, bit thirteen persons in various towns of the canton of Freiburg. All were recommended to be treated with one-twentieth of a grain of arsenic morning and evening, as a prophylactic measure. Eight submitted to this prophylactic measure, and none were affected. Four declined, or were not allowed, to take the arsenic. Of these four, two remained unaffected, and two died. One began the arsenical treatment, but speedily left it off. She was attacked, but at a much later period, and died. Dr. Guisan not only recommends the internal employment of the arsenic, but that the wound should be dressed with it. (*Correspondenz-Blatt*, August 1, 1872.)

Notes and Queries.

DEPARTMENT OF ANALYSIS AND INVENTIONS.

DR. CLERTAN'S PEARLS.—WHOLESALE DEPÔT, 49, HAYMARKET.

The various drugs are enclosed in small capsules, pearls, made of gelatine, which are to be swallowed like pills. In this manner the most nauseous drugs may be taken without discomfort. The envelopes of the pearls, although thin, weighing on an average only 1.75 grains, with a diameter of $\frac{3}{8}$ of an inch, enclose the contents hermetically.

ETHER.—The contents of these pearls consist of very pure sulphuric ether. Weight of contents: first pearl, 3.8 grains; second, 3.9 grains; third, 3.7 grains; mean weight, 3.8 grains, equal to 5.6 minims.

CHLOROFORM.—Contents, a very pure chloroform, spec. gr. 1.49. Weight of contents: first pearl, 6.6 grains; second, 7.0 grains; third, 6.4 grains; mean weight, 5.0 grains, equal to 4.8 minims.

TÉRÉBENTHINE.—Contents, pure oil of turpentine, having a boiling point of 156° C. Weight of contents: first pearl, 5.2 grains; second, 5.1 grains; third, 4.6 grains; mean weight, 5.0 grains, equal to 5.7 minims.

CASTOREUM.—The contents consist of an ethereal extract of castor. Weight of contents: first pearl, 4.2 grains; second, 3.0 grains; third, 3.8 grains; fourth, 3.3 grains; mean weight, 3.6 grains. The residue left on evaporation of the contents of four pearls amounted to 0.61 grains, or 0.15 grains per pearl, which may be taken roughly as representing from two to three times its weight of castoreum.

DIGITALE.—The contents are an ethereal extract of the dried leaves of the plant. Digitaline is present, if at all, in excessively minute quantity only, inasmuch as the contents of even six of the pearls do not yield sufficient to give the characteristic reaction, although this is a very delicate one. As digitaline is almost insoluble in ether, this is not, however, surprising. Weight of contents: first pearl, 3.2 grains; second, 4.3 grains;

third, 3·7 grains ; fourth, 4·2 grains ; mean weight, 3·8 grains. The residue left on the evaporation of the contents of four pearls amounted to 0·12 grains, or 0·03 grains per pearl.

ASSAFÆTIDA.—Contents, an etherial extract of the resin. Weight of contents: first pearl, 3·6 grains ; second, 3·8 grains ; third, 4·0 grains ; mean weight, 3·8 grains. Residue obtained from three pearls, 0·59 grains, or about 0·2 grains per pearl ; this may be taken as representing about twice its weight of the crude resin.

VALÉRIANE.—Contents, an etherial extract of valerian root, holding in solution chiefly essential oil and resin, and being almost entirely free from valerianic acid or its salts. Weight of contents: first pearl, 4·5 grains ; second, 3·7 grains ; third, 3·8 grains ; mean weight, 4·0 grains. Substance held in solution in three pearls, 0·09 grains, or only 0·03 grains per pearl.

On the whole it would appear from the foregoing that these pearls are admirably adapted for the administration of such drugs as ether, chloroform, oil of turpentine, &c., in which cases the whole of the contents consist of the active agent, but that they are little, if at all, suited for extracts, as the total quantity of active agent which they can hold in this condition is extremely small. In the preparation of these extracts a solvent must of course be employed which has no action on gelatine, and this necessarily limits the usefulness of these extremely elegant preparations.

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¹ Any of the foreign works may be procured on application to Messrs. Dulau, of Soho Square, W.C.; Williams & Norgate, of Henrietta Street, Covent Garden, W.C.; or Baillière, of King William Street, Charing Cross.

THE PRACTITIONER.

OCTOBER, 1872.

Original Communications.

THE INFLUENCE OF INHERITANCE UPON THE CURABILITY OF NERVOUS DISEASES.

BY DR. ANSTIE.

A QUESTION of great moment is incidentally raised by certain portions of the medical evidence lately given before Mr. Dalrymple's Committee of the House of Commons on Habitual Drunkards. One of the most important questions which were made the subject of inquiry was the extent to which the habit of intemperance was the result of a faulty nervous organization inherited from intemperate ancestors. Upon this point there was a large preponderance of opinions affirming that inheritance is not only a frequent but a very powerful factor in the development of drunken habits. As might partly be expected with a subject which the profession at large has treated with extraordinary neglect, the opinions of the experts have been much misunderstood. An editorial paragraph for example, in a contemporary medical journal (published at the time that the Committee was sitting), represented Dr. Skae as giving his opinion that the paroxysmal form of drinking, which is inherited, is more likely to be amenable to prolonged forced abstinence than is the form of drunkenness which is the mere result of sottish

habits gradually adopted under ordinary external temptation. Now that the evidence is published,¹ it is plain that Dr. Skae said nothing of the kind: the tendency of his testimony (though more hopeful as to the cure of paroxysmal drinking than I should have expected) was to assert that the fact of an inherited disposition made the cases more difficult, and more absolutely requiring a long period of detention and enforced abstinence, than those of ordinary tipping. I myself felt obliged to tell the Committee that I thought paroxysmal drinking was practically incurable, by incarceration or by anything else; that the sufferers from it were almost always members of families which were strongly tainted with insanity; and that the only paroxysmal drinker whom (in a considerable experience) I ever knew cured was a man in whose family the insane taint was very slight, if not absent.

The mistake, however, which the eager paragraphist of the journal referred to made as to Dr. Skae's meaning, might easily have been no mistake at all, for it represents a belief that is exceedingly prevalent, even among medical men who should know better, respecting the character of inherited tendencies, not only to drink, but to nervous diseases generally. It came out with startling force in an observation let drop by Sir W. Gull at a meeting of the Clinical Society last year. In criticising the history of a supposed cure of a neuralgia affection, Sir William suggested that the case was perhaps one of those slighter varieties of the disease, which are the result of neurotic inheritance, and that it might therefore have spontaneously ceased, instead of being cured by the remedies employed. Now in one shape or another I am continually encountering the same idea among medical men, viz., that the nervous diseases of persons who belong to neurotic families are slighter, and more spontaneously curable, than those which arise entirely from external circumstances in persons whose ancestors have been free from nervous diseases. How such an opinion first arose, there is some difficulty in understanding; and as I believe it to be directly contradicted by the results of careful observation, it may be worth while to investigate the matter a little further.

¹ See the Blue-book. "Report, Habitual Drunkards." Hansards. Price 2s. 6d.

It may be suspected that the example from which this notion of the mildness of inherited nervous diseases has been half unconsciously inferred by analogy, is that of inherited *syphilis*: yet even here it is plain that only a superficial view of the phenomena can have been taken. Granting the analogy between constitutional syphilis and the neurotic diseases, for the sake of argument, to be a fair one, then the whole of the phenomena of inherited syphilis should be looked at. Now it is true that, within limits, the tendency of inherited syphilis is to wear itself out, or at least to pass from the incurable into the curable stage. I suppose that there are not many English medical men who have had an extensive experience of constitutional syphilis, who would admit that we possess any means of treatment capable of really eradicating the disease from the system of the original recipient. Personally, I can say that nearly fifteen years' experience of dispensary and hospital outpatient practice has rendered any such belief impossible to me; so constantly have I found that the most completely treated, and apparently most perfectly cured subjects of constitutional syphilis, were liable to relapses if once their health became depressed below a certain point. On the other hand, the children of such persons, if they inherit syphilis at all, most commonly exhibit it in the form which occurs within a few weeks after birth; and that form is nearly always susceptible of permanent cure if it be promptly treated with appropriate remedies. But it must not be forgotten that in a considerable number of instances a more formidable variety of inherited syphilis makes its appearance at the time of the second dentition, and proves far more unmanageable than the kind which appears at an earlier period. Inherited syphilis of the period of second dentition is not frequently fatal; but it is singularly intractable to remedies, and the lesions which it causes are commonly permanent.

But in fact I need hardly say that the analogy between syphilis and nervous diseases is very imperfect. One would no more expect that the contraction of syphilis by a single individual would suffice to propagate syphilis in a permanent form to the race, than one would expect a single crossing of breed among dogs or horses to transmit *all* the characteristics

of the mixture of race to the progeny of the half-bred offspring by an animal purely of one or the other breed. And, though we are at present without any reliable evidence that the offspring of a person who inherited syphilis can transmit it (without a new infection) to the third generation, yet it may be that, in an increasingly modified form, at present unrecognisable by us, this does take place. If now we turn to the circumstances of neurotic inheritance, we find a far different set of conditions. The contagion of neurosis is exceedingly apt to renew itself by breeding in and in; and in fact this is constantly happening, by a kind of natural selection; for the members of families which are pervaded by nervous disease often possess quick wit and lively social manners, which cause them to be sought in marriage by their similars. Again, it is a peculiarity of the neurotic inheritance that it renders the offspring more liable to succumb to the influence of those very external causes which are most likely to confirm the nervous character of the constitution. Morbid intensity of application to some one branch of intellectual study; morbid impulse to emotional religious dissipation; or morbid shrinking from all grief and trouble, leading to excesses in drink, or opium, or sexuality: some one or more of these will probably tell with disastrous force upon a certain number of the members of any family in which already the nervous disposition has become distinctly manifest.

By these recruitments there is at last brought about that kind of constitution, in a family, which I have ventured elsewhere to call the Active Hereditary Neurosis, to distinguish it from the much slighter taint of nervous disease (called by me Dormant Hereditary Neurosis), in which we only trace the neurotic descent by isolated phenomena that are comparable to the sudden cropping up in a child of some peculiar cast of features which had not been seen in the family for several generations.¹ In the active hereditary neurosis, nervous disease of one kind

¹ It might be supposed difficult or impossible to prove that there really was any inheritance at all in such cases of disease. Experience, however, would soon convince the objector that it was real. The startling way in which a child will sometimes present the very image of a great grandfather who was utterly unlike the usual family type, is not more impressive than the manner in which epilepsy, or insanity, or facial spasm, &c., will break out in an individual in precisely the same circumstances and fashion as it assailed the grandfather or great grandfather.

or another is frequently appearing in a family, and those members who escape any definite disease are nevertheless marked by great mobility of the nervous system.

Let us now compare an individual of such a family, attacked with (let us say) neuralgia, with one in whom that disease has been set up in an accidental manner by external influences. [As I have elsewhere explained (*Journ. Mental Science*, January 1872), I doubt if there be ever a complete absence of hereditary influence in the production of neuralgia; but it may be of the Dormant kind, and the principal factors in the genesis of the disease may be some unusually severe and continuous external impression.] The former person, we will suppose, is a woman attacked with the pain, without obvious cause, about the age of puberty, or else at the menopause. The other shall be a woman in whom the affection has been produced by the continuous and simultaneous action of starvation and intense over-strain of function—*e.g.* ciliary neuralgia in a half-starved, hypermetropic needlewoman. Which of these two is most curable by direct interference? Why, the advantage is enormously on the side of the patient whose neuralgia has been artificially provoked: we can make her change her pernicious occupation, and we can order her iron and cod-liver oil, and (with the help of charity) some improvement of her food; and we expect to cure her entirely. But as to curing the spontaneous neuralgia, that is another affair. Where the hereditary evil influence is but slight, the neuralgia may wear itself out spontaneously; and in instances where the pain is even very severe, we may, by medicines and regimen, break the evil habit of pain. But not merely does the fact of inherited neurotic tendencies much increase the difficulty of conquering individual attacks, but the danger is great that such patients will be liable, all their lives, on the occurrence either of any of the natural physiological crises, or of unusual strains of fatigue of body or mind, to a recurrence of the complaint, as likely as not in increasingly severe forms. Above all, it is too probable that when the period for arterial degeneration sets in, the pain may recur with the persistency and intractability of the special neuralgias of old age, which appears to depend on permanent and incurable impairment of the nutrition of the nerve-root.

Of course I do not mean to say that there are *no* forms of neuralgia from external causes which equal or exceed in incurability the worst forms of the inherited disorder. But they are altogether exceptional. We may call the pressure on nerves from a cancerous tumour (which is necessarily continuous and incurable) an external cause of neuralgia. And among the various forms of nerve lesion from violence, there is one (which is altogether exceptional) that does not unfrequently set up a neuralgia at once of the severest and the most hopeless kind. The injury inflicted upon nerves by gun-shot wounds in modern warfare is something quite peculiar; and the account which Michell Moorhouse and Keen gave of the neuralgias resulting from such injuries received by soldiers in the American civil war, introduced us to a kind of disease obviously different from anything which is met with in civil life. In short, we may say this—that in order for any peripherally acting cause unaided to set up a neuralgia equally intractable with those in which the chief factor is a strong inherited neurosis, such peripheral cause must act with extraordinary violence, and establish an unusually permanent lesion of nerve-trunks. In all probability, the result of such severe injuries to nerves is serious degenerative mischief in the spinal centres, judging by the symptoms which the American authors describe.

Nor is the case different, if we look at other neuroses. Epilepsy, for example, when the outcome of an inherited neurotic disposition, is as nearly as possible incurable, although we have doubtless the power, in many cases, to mitigate its symptoms, and to render the patient's life comparatively tolerable, and even sometimes very useful. But not even the bromide of potassium, combined with all those dietetic and other regulations which are known to be of great subsidiary advantage, can be said ever to cure a well-established case of inherited epilepsy so that we could confidently pronounce that it would not relapse. On the other hand, there are a number of varieties of convulsions outwardly indistinguishable from true epilepsy, but wholly due to extrinsic sources of irritation; and in these, for the most part, early and suitable treatment is able really to control and put an end to the disease. For instance, if epileptic attacks be due to irritation of the brain from a roughened inner surface of the

calvarium, if it be possible to trephine and remove the offending substance, we expect to see the convulsive disorder altogether vanish with the immediate source of irritation. So, again, the practical physician thinks comparatively little of a *syphilitic* epilepsy, if the patient has a good, non-nervous, family history. He knows that he can perfectly control it by the use of iodide of potassium, or mercury, if he be summoned in good time; and although there is the possibility of relapse, that possibility is due to the tendency of syphilis to revive, not to any tendency in the nervous centres themselves. Even more striking, perhaps, is the difference between insanity that is inherited, and insanity that is created by external circumstances, as to curability. Every alienist knows this, and almost every ordinary practitioner of some standing must have met with one or more instances in which insanity produced by mere emotional shock in an otherwise healthy person, has been perfectly recovered from, and the patient has thenceforth remained free from any trace of mental alienation; whereas insanity occurring in persons who belong to decidedly neurotic families, seems to take a hold on the constitution which it never fairly relaxes. It is true that such a patient often recovers at the time, and some such persons continue to their lives' end without again being mad in a way that required actual confinement in an asylum; but in far the largest number even of such cases there is a stamp of eccentricity permanently imprinted upon the mind, and, one may even say, on the body too.

Undoubtedly, however, there are, in the case of insanity more especially, but also in that of other nervous diseases, certain concurrent causes which, where they exist, tend indefinitely to heighten the evil effect of the mere descent from neurotic ancestors, and which deserve to be specially singled out for remark, viz. (*a*) the inheritance of phthisis; (*b*) habits of self-abuse; (*c*) habits of alcoholic excess. As regards the first, there are many recorded facts, and many which I have personally witnessed, that show how disastrous it is for an already neurotic family to unite in marriage with a phthisical family; the offspring *may* be phthisical, but it is, at any rate, immensely probable that some of them will exhibit the neurotic tendencies of the stock with fresh and severe aggravation. Similarly, if an

individual descended from a neurotic, but otherwise healthy, stock, has the misfortune to contract phthisis in an *accidental* manner—*e.g.* as the result of a neglected catarrhal pneumonia—the new morbid element in his condition may indefinitely aggravate the already existing nervous tendencies. Such an event is sometimes the precipitating cause of an outbreak of furious and rapidly fatal delirious mania.

The influence of *self-abuse* comes in as a factor often of great, but on the whole of uncertain and varying, power, in aggravating the tendencies to nervous disease. Probably it is never the sole cause—as so many seem to think it—of the neuroses (especially epilepsy) with which it is often connected in the experience of those who see much of nervous diseases. Rather it seems likely that excessive masturbation, such as visibly and powerfully aggravates the tendency to atactic conditions of sensation and motion, is itself nearly always an expression of profound deterioration in the hereditary conformation of the nervous system. No one who has seen much of practice among children can have failed to meet with one or more of those frightful cases in which a child, well brought up and cared for, has exhibited the extreme type of *mauvais sujet* from a very early age; and it not very infrequently happens that the bent of such childish preternatural wickedness is in the direction of sexual matters. In many cases one cannot suppose such a phenomenon to be much, if it be at all, the result of any direct influence of companions. On the other hand, there is no doubt that even where there seemed no special proclivity to youthful vice of this sort, vicious habits have been often *taught*; and in any case the exhausting effect of premature sexuality upon the nervous system powerfully conduces to the aggravation of any existing neurotic tendencies.

The influence of *alcoholic excess* in heightening existing tendencies to nervous disease, and even creating them *de novo*, can be easily understood. There can be no doubt that this kind of excess most directly and actively interferes with the nutrition of the nervous centres, tending in fact towards the atrophy of the proper elements of nervous tissue, and the exaggeration of the mere connective-tissue elements of the nervous centres. But, moreover, the nervous centres of many drinkers are hereditarily unstable, hereditarily predisposed to break down in one

way or another. Hence the maximum of mischief is produced when such a subject takes to alcoholic excesses.

It seems, then, that the whole evidence afforded by the history of the neuroses points to the conclusion that of all the factors of these diseases which strongly tend to make them obstinate and intractable in type, there is none which is to be compared, on the whole, for efficacy, with the single fact of descent from a strongly neurotic family stock: and that of the subsidiary factors the most important are three (phthisis, self-abuse, alcoholic excess), which are eminently exhausting and depressing to the individual, and eminently calculated to transmit an enfeebled and badly self-nourishing organism to the offspring.

It must be confessed that, at first sight, the outlook suggested by these considerations seems gloomy enough. What business, it might be asked, has a journal of therapeutics, which should deal with practical matters only, to waste its space and the attention of its readers upon such an unprofitable theme? I think it is possible to answer such queries to the satisfaction of every thinking medical man. Reflect, once more, upon the problem presented to us. It is this: that we are called upon to seek for some means of breaking through an influence—that of heredity—which has shown itself to be one of the most powerful and inevitable of all the influences which assist in the production of serious and incurable nervous disease. We have found that the secondary factors which especially tend to develop to the utmost the pernicious power of the hereditary influence are such as do not come into operation (in regard to neuroses) to any great extent, *before the age of puberty*. Phthisis, masturbation, drink,—and I might have added false *educational* stimulation of the emotional part of the mind,—these are the last straws, either of which will break the camel's back; either of which, that is to say, is sufficient to develop the full pernicious influence of the inherited neurotic tendency. The fully developed mischief, at any rate, rarely breaks out before this period of life. Surely, then, the practical lesson is one of prophylaxis. We turn, to "education," in the fullest sense of the word, as the best prospect of mitigating those nervous diseases of which the hereditary character otherwise makes it so probable that they will be severe and intractable. We have

the whole pre-sexual period of life to work upon: a period during which it is possible to exclude the disturbing elements which most seriously complicate the problem of nervous disease. I shall not here repeat what I have said at some length in another place¹ as to the details of the plan by which it might be attempted, with good hope, to effect this prophylaxis: it will be enough here to point out the main directions in which it must be carried out.

1. The supply of a nutrition for young children practically unlimited except by the powers of digestion and assimilation: and the adoption of every plan (and especially graduated, but not excessively fatiguing, gymnastics) which may tend to increase both appetite and digestion.

2. The careful avoidance of all kinds of mental and spiritual training which would tend to generate self-consciousness, and the habit of unreal emotion.

3. The strict insistence upon a large allowance of sleep for children who are approaching the epoch of puberty.

4. Watchfulness, of the most unremitting kind, against the possible formation of bad habits, in conversation or act, with regard to sexual matters.

Strong as is the evil chain by which the burden of nervous weakness is laid upon the backs of the large and unhappy class who represent accumulated family tendencies to neurotic disease, we may fairly believe that in many cases it may be broken. For my own part, I am amazed to think how little the educational question has been yet studied in the sense of scientific prophylaxis against the nervous evils towards which individual children might be known to be specially prone. That this will be one of the great works of education in a not very distant future, I should be sorry indeed to doubt; and the present paper is one of many efforts which I would gladly make towards accelerating so desirable a reform.

¹ "Neuralgia and Diseases that resemble it." Chap. V. Macmillan, 1871.

THE TREATMENT OF CASES OF STRUMOUS OPHTHALMIA.

BY HENRY POWER, M.B.,

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THE frequency of strumous ophthalmia—known also under the names of phlyctenular, aphthous, and pustular ophthalmia, though they are not, strictly speaking, identical affections—and the difficulty in many cases of effecting a cure, are so great, that I think no apology is needed for calling attention to the value of a remedy, to wit belladonna, which I have found, when taken internally, to exert a remarkable influence over the progress of the disease in certain cases, when many other plans had been already tried, and which, though not new, has so completely fallen into disuse, that no mention is made of it in the most recent text-books on ophthalmic disease.

Phlyctenular ophthalmia is essentially, though not exclusively, a disease of childhood, the patients being for the most part from about four to twelve or fourteen years of age. It occurs usually in debilitated, ill-fed, or, if not ill-fed, ill-nourished children, both of the dark and fair type, though perhaps upon the whole more frequently of the latter. The mother will often volunteer the statement that it is the result of a prior attack of hooping-cough, scarlet fever, or, still more frequently, of measles, which left the child enfeebled, and liable to catch cold on slight exposure to cold winds or damp. After the attack of the exanthematous disease, the appetite has become variable, the bowels irregular, the evacuations being either scybalous, clay-coloured and very fœtid, or diarrhœa being present. Thread-worms are not unfrequently either a cause or concomitant of the disease. The face is pale or dusky, and the skin generally dry and hot, and there are often

enlarged glands in the neck. The child becomes languid and indisposed to join in amusements with its playmates, and the eyes are very liable to slight attacks of congestion or inflammation of a catarrhal nature. Herpetic eruptions about the nose, and discharge from the ears, are not uncommon accompaniments. The inflammation often attacks one eye, lasts for a week or two, and shifts to the other; and this kind of alternation may continue for many weeks or months. For a considerable time, especially amongst the children of the poor, little attention is paid to either the local or the general affection; but at length a more than usually severe exacerbation causes them to have recourse to medical advice.

The group of symptoms that are then presented are that, with the general aspect above described, the child exhibits intense intolerance of light; it holds the head down, covers the eye with the hands, and resists strenuously their removal; the least attempt to open the eyes causes pain, copious flow of tears, and not unfrequently sneezing, when a large quantity of muco-purulent fluid escapes from the nostrils; the lids are often almost natural, or but slightly red and swollen. To obtain a view of the cornea is no easy matter even to a practised hand, in consequence of the spasmodic action of the corrugator supercilii and orbicularis palpebrarum; and if it cannot be accomplished by placing the child with its back to the light and coaxing, nothing remains but to take a seat opposite the nurse, receive the head of the child between the knees, and raise the lid with a speculum. This proceeding, though perfectly harmless, is often regarded by the friends as a piece of refined cruelty, and it is perhaps better to administer chloroform, which has itself and alone sometimes appeared to me to produce a marked change for the better. It is advisable to make one thorough examination in the first instance, as occasionally ulcers of considerable depth may be found, which, unless properly treated, may perforate and leave an unsightly scar and great impairment of vision. Yet, in the majority of cases, the appearances of disease presented when the eye is fully exposed are wonderfully slight. A little redness of the conjunctiva, a small ulcer or two near the margin or on the surface of the cornea, sometimes with a leash of vessels running up to it, a superficial abrasion of the corneal epithelium, and

that is all. The cornea is elsewhere clear, the conjunctiva and sclerotic almost of their natural colour, the iris apparently healthy, with a much contracted pupil.

The cause of the intense photophobia, so out of proportion to the local inflammation, has occasioned much speculation. It is probably to be attributed to an exaltation of the sensibility of the optic nerve, resulting from irritation of the terminal branches of the corneal nerves. These branches we now know, from the researches of Klein, pass up between the cells forming the corneal epithelium, nearly, if not quite, to the surface, where it is probable they end by forming a plexus. When the epithelium is abraded, or, owing to defective nutrition, is not properly regenerated, the nerves must be exposed, and every motion of the lid must irritate them. Such irritation is believed to act reflectorally on the optic nerve-centres, producing photophobia, and also on the branches of the fifth, supplying the lachrymal gland, causing the profuse lachrymation.

The first stage in the development of one of the little herpetic ulcers, as they are often called, appears to be, as Iwanoff¹ has shown, a minute collection of nuclei or nucleate cells between the epithelium and the anterior layer of the cornea. These appear to be a proliferation of the corpuscles of the sheath of the nerves, or at least to travel along the nerves between the sheath of Schwann and the nerve substance, and thus form a pale vesicular-like swelling, which soon bursts, and leaves a small ulcer with yellow base, if on the conjunctiva, or with a whitish surface, and surrounded with an area or halo of grey, troubled or cloudy aspect, if on the cornea. In this state it may last for many days, neither materially increasing nor exhibiting any tendency to heal. In some bad cases, however, especially if the child's bowels and general health have been much neglected, it rapidly extends, and, perforating the cornea, leads to prolapse of the iris, after which the whole affection quiets down.

Wherever a disease is of a constitutional nature, and requires general treatment, directed by an intelligent appreciation of its etiology and of all the circumstances leading to its persistence,

¹ See an excellent paper on this subject in the *Medical Press and Circular* of November 29, 1871, by Dr. Swanzy, of Dublin, which came under my notice after this paper was written.

the remedies employed must necessarily be various, because each case requires to be treated upon its own merits, and the sound practitioner finds that he must vary his treatment in accordance with its exigencies; and also because there have always been those who, mistaking the *post hoc* for the *propter hoc*, have attributed to the action of some drug or agent acting locally, which has really been due to other circumstances acting generally. Hence alike the origin of and the disappointment occasioned by specifics. The number of remedies recommended to be employed in cases of phlyctenular ophthalmia is singularly large. Looking over comparatively few books, I find white and yellow precipitate of mercury, nitrate of silver, calomel, belladonna, tartarised antimony, opium, lead lotion, borax, aqua laurocerasis, tinctures of iodine, of galbanum, rhus toxicodendron, faba ignatii, hydrocyanic acid, cyanide of potassium, charophyllum sativum, dipping the head in cold water, blisters, setons and issues, and even division of the supra-orbital nerve, proposed as local remedies; whilst purgatives of all kinds, acorn coffee (infusion of it), the green husks and shell of the walnut, potassio-tartrate of antimony, chalybeates, preparations of iodine, cod-liver oil, quinine, turpentine, senega, conium with carbonate of magnesia and chloride of barium, hyoseyamus and bignonia catalpa, recommended as appropriate means for combating the constitutional disturbance. And no doubt, under certain circumstances, the use of each of these may have been followed by improvement; whilst in some cases each may be tried without the slightest benefit.

Hospital out-patient cases often prove very refractory; but this is readily explicable when the food they consume, the little exercise they can take, and the air they habitually breathe, are taken into consideration. It appears to me that in the treatment of this disease, attention should always in the first instance be directed to the state of the bowels. It is advisable to see the motions, for valuable indications of treatment may be derived from them. As a rule a brisk purgative is very useful at the outset, and I generally prescribe five or ten grains of the compound jalap and scammony powder, with one or two grains of calomel, which, if the scammony be of good quality, acts well. An emetic repeated for two or three mornings, as suggested by Mackenzie, sometimes acts capitally. Ten or twenty grains

of ipecacuanha may be given with half a grain or a grain of potassio-tartrate of antimony, according to age and strength. When the bowels are quite natural, some form of tonic is required, and our choice usually lies between the preparation of iron, quinine, and cod-liver oil. The former may be given in rather large doses, as from ten to fifteen minims, the quinine in one or two grain doses, and the oil in one to four drachm doses, three times a day. The latter remedy acts best in children of fair complexion. Great advantage will be found in adding two or three grains of hydrargyrum c. cretâ to the quinine. For some years I always commenced the treatment by giving this combination to children suffering from strumous ophthalmia, using no applications to the eye except atropine, and was well satisfied with the results.

General treatment, however, is by no means sufficient to cure the patient, and amongst the many local remedies I have tried, I should give the palm to atropine, in a two- or four-grain solution, Pagenstecher's yellow ointment, and calomel. With one or other of these most cases may be cured.

Cases, however, occasionally occur in which all these plans of treatment fail; and the question comes, what must now be tried? It is then that I claim attention to the value of extract of belladonna, given internally. I have repeatedly found that it rapidly diminishes the intolerance of light, and by its power of relieving the spasm of the muscles closing the lids, enables the child to obtain an amount of benefit from air and exercise that was previously impossible. I can entertain no doubt that its good effects are attributable to its action as a stimulant upon the sympathetic system of nerves, and through this upon the smaller vessels. It is further of use in doing away with the necessity for purgatives, as even in small quantities it acts efficiently in clearing the bowels. I have usually prescribed it in doses of one-eighth to one-quarter of a grain. It is perhaps scarcely necessary to add that, as it is a potent remedy, its effects must be watched, and its administration should be suspended as soon as the child complains of thirst, or when the rapidity of the pulse is observed to be increasing. I have only noticed these symptoms in one or two instances. I have also found the extract of belladonna serviceable in cases where the affection was rather a

limited keratitis than phlyctenular ophthalmia; that is, in which a small segment of the cornea was hazy and vascular near the margin, even though the intolerance of light may not have been very intense. I consider the seton, though recommended by so good an authority as Mr. Bader, a *pis aller*; and find the treatment mentioned by Dr. Swanzy, as practised by Von Gräfe, though it was originally suggested by Jüngken, of dipping the whole head for a few seconds in cold water, not persistent in its effects.

The following is a *résumé* of one of the cases that has recently been under my care, showing the benefit that may be derived from the use of the extract of belladonna in obstinate phlyctenular ophthalmia:—

M. C., æt. 4, was brought to me in the beginning of January of the present year. She had had measles (not an unusually severe attack) in August 1871; this was followed by stoppage in the nostrils and herpetic eruption on the lip and on the right ear. At the end of August her mother took her to Yarmouth for a month. She was ailing all the time she stayed there, complaining of thirst, with restlessness at night, languor, and loss of appetite. These symptoms continued up to November, when the right eye became affected. She was taken to an ophthalmic hospital, when some tonic medicine was ordered for her, and a poultice was directed to be applied. A crop of pustules appeared over the brow, temple, and cheek, and the left eye became affected. I then saw her, and found that she was suffering from a well-marked attack of phlyctenular ophthalmia. On examining the eyes several pustules were visible on the conjunctiva at the margin of each cornea, whilst superficial abrasions or ulcers were apparent on the corneæ themselves, the right being much the more severely affected, so that a large part of its surface was cloudy. She was ordered a purge, and a powder containing three grains of hydrargyrum c. cretâ with one of quinine, thrice daily. A four-grain solution of atropine was dropped in, and a lotion of extract of belladonna given to take home, and to be used frequently. But little improvement resulted, though the mercury and quinine were continued for a fortnight. The bowels being somewhat disordered, and the lips and ala nasi having some pustules upon them, it was thought likely that there

were worms. Another purge of compound jalap powder and a turpentine injection were therefore ordered. No worms, however, were observed in the motions; cod-liver oil was now prescribed, and after a short time a little steel wine was added to it. The use of the belladonna lotion was continued. This plan was persevered in for a month with little benefit; the child sometimes opening the eyes in the evening and playing about, but always coming to me in the morning with intense photophobia. The mother was exceedingly intelligent, and assured me that the bowels were regular, the motions natural, and that the food given to the child was carefully selected, wholesome in quality, and though its appetite was variable, yet upon the whole the quantity was sufficient for its needs. Calomel powder was now applied directly to the cornea for several days, but ineffectually, so far as the relief of the photophobia was concerned.

Finding that the symptoms were stationary, I now determined to tap both eyes with a broad needle, and allow the aqueous humour to escape. The benefit was considerable, the child opening the eyes on the following days for some hours, and I thought recovery would take place without further trouble. However, a relapse soon took place, and she was now placed upon one-sixth of a grain of good extract of belladonna three times a day. In two days the mother brought her to me obviously greatly improved. The intolerance of light had greatly diminished, and she even saw to pick up a pin on the carpet. She continued the use of the extract for a fortnight, when it was given up, and some quinine mixture ordered. A few days later she went into the country. This child has lately been seen by me, looking fresh and rosy, with only slight nebulae on the corneae of both eyes, but able to bear strong light perfectly; and I can feel no hesitation in attributing the improvement here observed to the internal use of the belladonna, it was so immediate and proved so persistent.

THE PHYSIOLOGICAL ACTION OF CAMPHOR.

BY DR. JOHN HARLEY,

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MOST of us prescribe camphor daily ; and while all of us regard it as a cleanly and palatable vehicle for more active drugs, few of us, I presume, have any very definite views of its medicinal properties. The usual dose is about half a grain (= $\bar{3}j$. aquæ camphoræ¹) every four hours, making a total of about 3 grains in the twenty-four hours ; and we rarely or never prescribe more than 5 grains for a single dose. Now, since 5 grains are insufficient to produce any appreciable symptoms, it follows that as a rule the majority of medical men have never observed the physiological effects of camphor. The object of this paper, therefore, is to set the medicinal effects of camphor in a clear light. I have been induced to test the therapeutical value of the drug in a few cases, and in these I have had an opportunity of learning something of its physiological action when given in doses which have been reported as poisonous. As I wish to give as complete a view as I am able of the action of camphor, it will be necessary and I think interesting to give a brief account of a few recorded cases of poisoning by camphor before I lay before my readers the results of my own imperfect observations.

The following cases are for the most part to be found scattered through our works on Toxicology and Medical Jurisprudence. In selecting them I have been careful to discriminate between the real effects of camphor and those due to other causes. How necessary it is to do this will be seen from the following instances of so-called poisoning.

¹ 1,000 grains of water take up about 1 grain.

CASE I.—A young woman, aged 27, suffering from nervous symptoms, “neither epileptic nor hysterical,” received as an antispasmodic 4 grammes, = 60 grains, of camphor. Two minutes after the enema the patient complained of a sense of fainting, and pain in the abdomen; she was then seized with a violent convulsive attack, attended with loss of consciousness, foaming at the mouth, &c., and extreme distress in respiration. The paroxysm lasted twelve or fifteen minutes, when the patient recovered consciousness. A purgative enema was given to remove the camphor, and other remedies applied to raise “a feeble pulse,” relieve “the distress in breathing,” and to calm “the agitation and anxiety of the patient,” and at the end of four hours after receiving the enema she was out of danger.¹

It is to be observed that the symptoms came on two minutes after the introduction of the camphor, and it is quite plain (as will appear from all the other cases) that they were not due to the absorption of camphor, which could not take place in the time. The fit probably differed little or nothing from those to which the patient was liable. The rest of the symptoms must be regarded as the after effects of the epileptiform attack.

CASE II.—A villager of Neudorf, in Strasburg, gave to each of her two sons, one of three and the other of five years old, and to her infant, eighteen months old, a dose of about a tablespoonful (180 grains) of powdered camphor as a vermifuge. After half an hour, nausea, vertigo, and muscular twitchings came on. After an hour and a half, vomiting and convulsions followed, and then loss of consciousness and a frequent “desire” to pass urine. The three little patients were simultaneously the subjects of violent convulsions, in which the facial muscles were implicated, and they were tormented with incessant vomiting, purging, and voiding of urine. The attack lasted three hours, and was followed by a comatose sleep of several hours’ duration. The means employed were *emetics*, *purgatives*, and *narcotics*. The urine had the odour of camphor. The infant died within seven hours, comatose; the other two soon recovered.²

¹ M. Aran, Communication to Med. Soc. of Parisian Hospitals, Lond. Med. Gaz. vol. xlviii. p. 552.

² M. Schaaf, Journal de Chimie Médicale, 1850. Lond. Med. Gaz. vol. xlvii. p. 219.

Dr. Taylor¹ also refers to vol. xlvii. of the *Medical Gazette* for the account of this case, and yet he states that the dose given to each child was about 30 grains (half a teaspoonful). I have been unable to refer to the *Journal de Chimie Médicale*, but even granting it was the smaller dose, the presence of worms in, and the administration of a teaspoonful of powdered camphor to, a child 18 months of age, are two statements difficult to accept. Incessant vomiting, purging, and vesical irritation are not (as will appear from all the other cases) the effects of camphor. Vomiting was no doubt caused by the emetics, purging by the purgatives, and, it may be, coma and death by the narcotics employed in the treatment of the patients.

CASE III.—An adult male took 30 grains of camphor as an enema. After a few minutes he perceived a taste of camphor in the throat. The enema having been retained a quarter of an hour, he became uneasy and generally unwell. On rising from his bed he was surprised to find that he felt lighter than usual, and he seemed to skim along the ground as he walked, and he staggered in walking. He then appears to have become alarmed by these symptoms, and felt weak and faint. He took a glass of wine, and the symptoms, which had continued for about half an hour, gradually passed off. During the rest of the day he exhaled by the mouth a strong odour of camphor.²

In this case the symptoms appear to be chiefly due to alarm caused by the retention of the enema, and the slight giddiness induced by the camphor.

CASE IV.—A child, suffering from cerebral disease, had been for some hours lying perfectly senseless and quiet, with a slight flush on the cheeks, squinting, pupils dilated, and the pulse 64, intermitting every fourth stroke. In this state Mr. George determined to try the effects of camphor on the heart, and in order thereto enveloped the chest in flannel soaked in a solution composed of one part of camphor and two of rectified spirit. In less than five minutes the child became pale, moaned, and appeared restless and distressed, the pulse beating so rapidly that it was

¹ Pereira's Elements Mat. Med. vol. ii. Part II. p. 456, and Med. Jurisprudence, 5th edition, p. 177.

² Orfila, Traité de Toxicologie, 4th edition, vol. ii. p. 496. Quoted by Christison, p. 909.

almost impossible to count it. These effects increasing, the application was removed, and the child returned in a few hours to the condition in which it was before the camphor was applied, and died in about twenty-four hours.¹

This case is recorded to show the stimulant effect of camphor on the heart. It hardly need be said that the cardiac excitement was due to the pain and bodily disturbance caused by the application.

CASE V.—An adult patient, in the enjoyment of perfect health otherwise, inhaled the vapour of camphor for some hours in order to cure a catarrh. After a time he felt himself overwhelmed with a general and unaccountable lassitude. The head became heavy, stupid, and painful, the vision disordered, and there was shivering at intervals. Soon he was unable to stand; faintings and nausea supervened; and the physician, M. Journez, found him in a state of extreme prostration, pale, and trembling, with a tendency to slight faintings, a slow soft pulse, and the breathing quick and irregular. He soon recovered, and the urine exhaled a very marked odour of camphor. Previous to inhalation the camphor was moistened with alcohol.²

In this case the shivering, nausea, and faintness were probably due to alarm.

In the following cases the symptoms recorded are purely those induced by camphor.

CASE VI.—Mr. Alexander took 20 grains of camphor, and finding that it did not cause any particular symptom, swallowed, on another occasion, 40 grains mixed with syrup of roses. In the course of twenty minutes he became languid and listless, and in an hour giddy, confused, and forgetful. Objects quivered before his eyes, and a tumult of crude ideas floated through his mind. At length he lost all consciousness, during which he was attacked with strong convulsive fits and maniacal frenzy. These alarming symptoms were dispelled by an emetic, which brought away almost the whole of the camphor which had been swallowed three hours before.³

¹ London Medical Gazette, vol. ix. p. 662.

² Archives Belges de Méd. Militaire, and Journal de Chimie Médicale, 1860, Fourth Series, vol. vi. p. 466.

³ Experimental Essays, p. 128, quoted by Christison "On Poisons," 4th edition, p. 909.

CASE VII.—An adult male took 40 grains of camphor dissolved in olive oil. Vertigo, cold extremities, great anxiety, cold sweating of the head, slight delirium attended with somnolency, and a small languishing pulse, were the symptoms at first experienced, but they were soon followed by great heat and a quicker pulse.¹

CASE VIII.—A drunkard swallowed $\bar{\text{z}}$ iv of camphorated spirit, = 160 grains, of camphor. Soon afterwards he became feverish, with burning heat of the skin, burning pain in the stomach, giddiness, flushed face, dimness of sight, and some delirium. The poison was retained, but the man soon recovered.²

CASE IX.—A weak, nervous woman, aged 36, took 180 grains of camphor dissolved in a glass of brandy, with the view of producing abortion, being about four months pregnant. During the first hours which followed its ingestion she experienced symptoms of intoxication, headache, flushing of the face, and a sensation of heat in the stomach; but eight hours afterwards she began to suffer pain, at first moderate, but afterwards very intense, in the epigastrium, radiating all over the belly and into the limbs. This was accompanied by uterine tenesmus. She continued very ill with symptoms of peritonitis, and died on the twelfth day, having aborted just before her death.³

The camphor appears to have set up uterine action in this case. The symptoms which followed are not to be attributed to the camphor.

CASE X.—A female adult took fasting, at 8 A.M., in mistake for castor-oil, two tablespoonfuls of camphorated oil (*camphoræ $\bar{\text{z}}$ ss, olei olivæ $\bar{\text{z}}$ j*). At 11 A.M. she was delirious, but when spoken to gave rational answers: she said she had no pain, but her head turned round. Her face was pale and anxious, the pupils dilated, the hands and feet cold; the pulse 120, and feeble. At 12.30 the pulse was 108 and feeble, and she felt weak. In the evening she was quite recovered, and the pupils and pulse were natural. "During no part of the time was the respiration disturbed."⁴

¹ Orfila, *op. cit.* p. 496.

² Wendt, *Rust's Mag. für die gesammte Heilkunde*, xxv. 88. Christison, *op. cit.* p. 910.

³ *Gazette Médicale d'Orient. Bull. de Thérapeutique, and Journal de Chimie Médicale*, 1860, Fourth Series, vol. vi. p. 21.

⁴ Alex. Stookes, *Med. Times*, 1848, vol. xviii. p. 83.

I will now give in detail an account of two cases in which camphor was freely employed. It is to be observed that the physiological effects were uniform, and that in doses not exceeding 35 grains there were simply giddiness, languor, and a diffused feeling of warmth through the body—the digestive, circulatory, with slight exceptions, and respiratory apparatus remaining unaffected.

The following solution of camphor was used on every occasion :—

R Camphoræ ζ iv.

Aquæ ζ ij.

Spiritûs vini rectificati ad $\bar{\zeta}$ vj. f. $\bar{\zeta}$ j = gr. v camphor.

The dose was taken in one or two tumblerfuls of warm water, which dissolved the greater part of the camphor, except in the case of the larger doses.

CASE XI.—*Dysmenorrhœa ; chronic ovarian pain from congestion of the pelvic viscera ; camphor in doses varying from 2 to 30 grains.*—Mary L., aged 37, single, moderately dark, and spare and weakly. The catamenia appeared at the age of 14, and have continued at regular intervals up to the present time ; the discharge was always rather scanty, and continued for three or four days ; during the last few years the discharge has been interrupted on the third day, reappearing on the fifth, and then continuing for one or two days. For the last ten years she has had “ a lump of dragging, aching pain ” on the left side, extending from the navel to the groin, and settling deep in the left side of the pelvis. For the few days following the catamenial periods she is relieved, but the pain soon returns, and gradually increases in severity until the menstrual discharge again sets in. During the flow there is much aching pain in the back. The bowels are habitually confined, and the pelvic congestion is occasionally relieved by bleeding from internal hæmorrhoids, and commonly by leucorrhœa. By the use of cold-water enemata, laxatives, henbane in large doses, and chalybeate tonics, her general health was improved, and the local disorders relieved ; but at the end of ten months the pelvic (ovarian?) pain continuing, I suspended all other treatment, excepting the means employed for promoting regular action of the bowels, and for the next four months administered camphor alone. At this time the uterus

was healthy, and the cervix readily allowed the passage of a sound.

Two grains of camphor were taken in solution, as above, every day for the first week, on an empty stomach. The report was that the medicine did not affect her in any way, and the following observation corroborated this statement:—At 11 A.M., the pulse being 72, regular, soft, and rather voluminous, the tongue moist, with a slight white fur, and the pupils in diffuse daylight $\frac{1}{8}$ ", she took her first dose, and for the next two hours sat down and plied her needle. *After half an hour* the pulse was 62, but otherwise unchanged. *After one hour and a half* the pulse was 60, and unchanged. *After two hours* the pulse was still 60, regular, and of unchanged volume and power. There was no change in either pupils or tongue throughout.

Four grains of camphor were taken every morning for the next fortnight, an hour before breakfast. She stated that the medicine produced a sense of warmth through the whole body, and that it neither increased nor diminished her appetite.

Five grains of camphor were given to her two and a half hours after breakfast, the pulse being 64, the tongue clean, and its secretion faintly acid. *After an hour and a half* the pulse was 60, unchanged, the tongue and pupils unchanged, and no other effect experienced except the diffused sensation of warmth—differing from that induced by exercise, and especially referred to the chest and breasts.

During the fourth and fifth weeks, five grains of camphor were taken in the morning before breakfast, and again at bedtime. She continued her usual domestic occupations after the morning dose, and reported that she experienced decided giddiness after each dose, coming on after about twenty minutes, and lasting ten minutes. On two occasions the giddiness was considerable.

Ten grains of camphor were taken two and a half hours after breakfast, the pulse being 60. *Half an hour afterwards* she felt very giddy and powerless: this passed off in the course of ten minutes. *An hour and a half after the dose* she experienced no effects, and employed her time in reading. Pulse 58, unchanged. She continued to sit quiet, and *at the end of two*

hours and a quarter the pulse was 62, regular, and of the initial volume and power. The tongue and pupils were unchanged throughout. There was a general feeling of languor.

On another occasion the observations were continued until four hours after the dose. At the end of this time the pulse, pupils, and tongue were unchanged, and no effects of any kind were felt.

During the sixth week she took ten grains of camphor every morning before breakfast, and reported that each dose produced so much giddiness that she was scarcely able to walk across the room. The effect came on after ten minutes, continued at its maximum for the next quarter of an hour, and then declined, but did not pass away entirely until after breakfast, *i.e.* one hour and three-quarters after the dose.

During the next three weeks she took *fifteen grains of camphor* every third morning before breakfast: each dose produced giddiness in from five to fifteen minutes, lasting at its maximum a quarter of an hour, and continuing for an hour and a half. There was no disorder of vision, and she could continue reading when the giddiness was at its maximum. Giddiness was the only appreciable effect: there was no feeling of stimulation nor faintness, and the appetite was unaffected. She took fifteen grains under my own observation two hours and a half after breakfast, the pulse being 70, and the pupils (a sunless day) $\frac{1}{4}$ ". *Half an hour afterwards* she complained of the usual giddiness, but said it was beginning to pass off. She has continued her sewing uninterruptedly. Pulse 70, of slightly increased volume and good power, and quite regular. *After an hour and a half*, pulse 63, of initial volume and power; giddiness nearly passed off, but the legs felt rather tremulous in walking. She now felt sleepy. The giddiness induced by the medicine did not dim the sight, but things did not appear quite steady before her eyes; there was a little confusion when she looked up. The pupils and tongue were unchanged throughout.

During the next fortnight no medicine was taken. From the thirteenth to the fourteenth week, camphor, in doses of ten grains, was taken twice a day. It still continued to induce giddiness, more so in the morning before breakfast, and frequently "made her very warm."

Twenty grains of camphor were given to her two hours and a quarter after breakfast, the pulse being 72, the pupils $\frac{1}{8}$, and the tongue natural. *After twenty minutes* she was very giddy, but could stand, and stoop to pick up a pin. Pulse 68 and unchanged. She continued her sewing, but shortly afterwards she was obliged to lay her needle aside, not so much on account of disorder of vision as of tremulousness of the hands; and there was slight somnolency. *After two hours and a quarter* she was still a little shaky and unsteady in her actions, and felt languid and sleepy: the pulse was 66, regular, and of its initial force and volume: the tongue and pupils were unchanged throughout. She now walked home. During the next week she took ten grains twice a day for three successive days, and on the fourth day—

Thirty grains of camphor, ten grains an hour before breakfast, and twenty grains two hours and a quarter after breakfast, the first dose being taken at 7 A.M., and the second at 10.15 A.M., when the pulse was 70, the tongue clean and moist, and the pupils at bright light $\frac{1}{16}$ ". The giddiness and muscular weakness were rather less marked than after the single dose of twenty grains. She was able to continue her sewing throughout, and at the end of two hours, when she went home, the pulse was 60 and unchanged, and the tongue and pupils were unaffected.

During the following week she took ten grains thrice a day. At the end of this time she was looking well, and the lips were rosy; she however seemed languid and lethargic, and reported that the medicine made her languid and sleepy. Throughout the week she was excessively weak and languid, her legs being weak and shaky, and she was hardly able to keep her eyes open during the day: twice she had felt very faint, and she had constantly experienced a heaviness and dulness of the head, which "seemed so bewildered." There was no dimness of sight, but the vision was "flickering and weak;" in fact she was decidedly camphorised (see experiment on the mouse); the pulse was 72, regular, and of its usual volume and force; the respirations 23 one minute, and the next 19, with one long-drawn inspiration. Throughout, the digestive, circulatory, and secretive organs were apparently unaffected; the urine was not increased in quantity, or in any way altered

and I uniformly failed to discover the odour of camphor in this or in the pulmonary or cutaneous exhalations.

During the four months she continued the camphor treatment there was decided amelioration of the dysmenorrhœa, the flow being interrupted only on one occasion, and the pelvic pain and flatulent colic were greatly relieved. As soon, however, as the camphor was discontinued all the symptoms returned.

In this case the camphor acted beneficially as an anti-spasmodic.

CASE XII.—*Frequent seminal emissions; camphor in doses from four to thirty-five grains.*—Henry A., aged 17, a strong, active young man, troubled with seminal emissions twice a week, and the usual nervous symptoms. He was greatly relieved by hemlock, ʒvj of good succus producing moderate cicutism. After an interval of some months he again applied to me, and this time I prescribed camphor. He took two doses of four grains each; after an interval of a week, seven doses of five grains each, on consecutive days; and for the next six days, five grains night and morning. The following week he took ten grains every morning before breakfast, and I then increased the dose to fifteen grains, which was taken every other morning for a week longer. During the next two months he took a dose of camphor, progressively increased from eighteen to thirty grains, at intervals of two or three days; and for four consecutive days during the latter part of the time he took thirty grains night and morning. The effect on the irritation of the sexual organs was marked, emission occurring only once a fortnight during the time the camphor was taken. No dose less than fifteen grains produced any appreciable effect. When this quantity was taken before breakfast, it caused giddiness after fifteen minutes, disappearing in the same interval of time. Giddiness lasting a quarter of an hour, and a warmth of stomach “as if he had taken spirits,” followed doses of twenty grains. The following observations show the effect of twenty-five, thirty, and thirty-five grains respectively:—

Twenty-five grains of camphor were taken when the pulse after walking was 79, the pupils $\frac{1}{8}$ ”, and the tongue clean. *After twenty minutes* there was giddiness, but the patient did not show it, and a warmth about the chest; the pulse 76.

The giddiness lasted half an hour. *After two hours* the pulse was 73, regular, and of its usual volume and power; the pupils and tongue were unchanged throughout, and although he had been sitting still in a darkish room during the whole of the time, there was no somnolency.

Thirty grains of camphor were taken when the pulse, after a walk, was 88, full, and regular. The usual giddiness came on after a quarter of an hour, and continued for half an hour; to me it was not appreciable. *After thirty-five minutes* the pulse was 76, and had returned to its usual volume and force for a state of quietude; a diffused feeling of warmth throughout the body, but especially behind the sternum. *After an hour and three-quarters* the pulse had risen to 86, but was otherwise unchanged; the other symptoms had passed off, and there was no somnolency.

Thirty-five grains of camphor were taken after a walk, when the pulse was 86, and the pupils $\frac{1}{4}$ ". Giddiness "over the back and top of the eyes" came on in a quarter of an hour, and continued at its maximum for twenty minutes. There was no defect of vision. *After thirty-five minutes* the pulse was 76, full, and bounding; the cheeks hot and flushed, and there was a little somnolency: no change of pupils or tongue, and a sensation of warmth over the upper part of the chest. *After two hours* the pulse was 76, regular, and of its usual force and volume; the somnolency and flushing were gone, but he still felt heavy about the eyes. He now walked home, and experienced slight giddiness on reaching home, lasting half an hour.

No greater effects were produced when thirty grains were taken twice a day.

The urine was examined after the operation of the medicine on each of these occasions; it was always normal in quantity and composition, and it possessed its natural odour, being entirely free from that of camphor—(these observations equally refer to Case XI.)—nor was the odour of camphor in the least degree appreciable in the cutaneous or pulmonary exhalations of either patient. The respiration was not affected to any appreciable extent in either case.

I will now complete my observations on the physiological

action of camphor by a brief account of its effects on the moth and on the mouse.

EFFECTS OF CAMPHOR ON THE MOTH.

The small grey-spotted moth, the black hairy larva of which devastates the hawthorn in London during the spring, was used in the following experiments :—

A pint beaker, containing eight moths, was inverted over a lump of camphor the size of a split pea, the temperature of the room being 70° Fahr. *After fifteen minutes* all were rolling and fluttering upon the table, being now unable to retain foothold upon the glass. *After thirty minutes* all were perfectly still, and some lying on the side, but on shaking the stand they moved, struggled to keep on their feet, but most rolled over on their backs. They continued in a torpid state, faintly struggling and rolling about only when disturbed. *After eight hours* all were alive, and one-half lying on the back. *After twelve hours* five were dead, and the three remaining ones were on their backs, and incapable of changing their positions. The beaker was now removed, and they were freely exposed to the air. *After sixteen hours* they were in no degree revived, and next day two were dead, and the remaining one was crawling about pretty actively.

The experiment was repeated when the temperature of the air was 8° higher; namely, 78° Fahr. They soon began to flutter about, and, losing hold upon the glass, fell upon the table and became torpid. In the course of a few hours all were dead.

EFFECTS OF CAMPHOR ON THE MOUSE.

Five grains of powdered camphor were mixed with twice their bulk of fine sand, and placed in a muslin bag. The bag and a wild active male mouse were simultaneously placed under a glass shade of the capacity of two cubic feet, the shade being raised a quarter of an inch from the support to allow of the circulation of air, the temperature of which was 70° Fahr. The camphor vapour caused at first a little blinking of the eyes, but in the course of an hour the animal became dull and sleepy, and lay perfectly still, with the eyes half closed, the chin resting on the table, and the respirations a regular pant of 200 in the

minute. He continued in a tranquil and apparently comfortable dozy or sleeping condition for the *next twenty hours*, being aroused at intervals of half an hour or an hour by yawning or irritation of the skin, which, after some hours, became decidedly pink. When dozing or sleeping, his condition became so lethargic that he allowed himself to be pushed about or taken up; but when aroused by this interference, or by his own sensations, he was able to run nimbly and rear himself on his hind legs, and in this state the intelligence and senses were apparently intact. Left to his own sensations, he rarely moved, and when he did so the motions were most ludicrous; the hind-quarters were left behind as the parts in front were slowly advanced, until the body was elongated to twice its usual length, and then, as the haunches were drawn forwards, the animal took the opportunity of stretching one of the hind legs behind him, and sometimes accompanied the act by a corresponding stretch of the neck and elevation of the head, and finished the lazy performance with a prolonged yawn. Thus aroused, he would sit on his haunches and scratch and rub himself from head to tail. Occasionally he took up a bit of cheese and nibbled it lazily. There was no desire for water. Fæces and urine were passed as usual, and in normal quantity. He continued dull, and refused water twenty-four hours after the camphor was removed, but afterwards became as active and lively as ever. Independently of the slight cutaneous irritation, the effect throughout appeared to be that of a grateful soporific.

It appears from the foregoing that camphor exerts its action chiefly upon the cerebral lobes, causing at first depression of mental power, giddiness, and somnolency. The corpora striata appear to share the general sedation of the intellectual centres. Delirium comes on later, and in some cases there is considerable vivacity. If the use of the drug be continued for some time, it produces great depression of muscular power and intellectual lethargy. In the fullest medicinal doses it does not affect any of the organic functions, excepting such depression of the sexual as may fairly be considered a secondary effect of its depressing influence on the motor and intellectual centres. In all medicinal doses from the lowest to the highest it certainly does not exercise a depressant effect on the circulation. On the contrary, decided

stimulation is occasionally to be observed after large doses, and this is attended with a diffused feeling of warmth throughout the body, and a slight rise of the temperature of the surface. Given in solution, as above, I have every reason to believe that the camphor was rapidly and completely absorbed; and in the doses above mentioned, it seems to be as rapidly and completely decomposed, for I always failed to detect a trace of camphor odour in either the urine or the exhalations from the skin and lungs.

SOME ADDITIONAL OBSERVATIONS ON THE ACTION OF ATROPIA ON SWEATING.

BY SYDNEY RINGER.

I MADE these observations, with the assistance of Mr. Johnson and Mr. Curtis, resident officers of University College Hospital, to learn the smallest quantity of atropia injected hypodermically required to check sweating.

We injected $\frac{1}{300}$ of a grain of atropia into the arm of a man aged 60, while he was sweating profusely in the hot chamber of the Turkish bath. In fifteen minutes the sweating became considerably less, but after nineteen minutes the perspiration returned abundantly, but less than before the injection. He perspired freely after the cold needle bath.

We placed a lad in a hot-air bath of 194° Fahr., and when perspiring profusely we injected $\frac{1}{300}$ of a grain of atropia. In five minutes the perspiration was decidedly less, and in ten minutes was very slight, but in thirteen minutes it again became profuse. Then we injected another $\frac{1}{300}$ of a grain, and in two minutes his face became perfectly dry, and remained so during the rest of the *bath*, i.e. ten minutes.

We next injected $\frac{1}{200}$ of a grain into the arm of a lad while sweating profusely in the hot chamber of the Turkish bath. Four minutes after the injection his body was quite dry, but subsequently slight moisture appeared on his forehead. After the cold needle bath, his skin remained perfectly dry.

Our next observations were made on patients troubled with profuse sweating, especially at night. The first patient suffers from a renal tumour, with discharge of pus in her urine. She was free from fever, neither was she very weak. The profuse

sweating caused her great annoyance. On August 30 we injected hypodermically $\frac{1}{100}$ of a grain of atropia. This completely checked the sweating on that and the following night. On Sept. 1 she was sweating again very freely, and $\frac{1}{200}$ of a grain was injected, which effectually prevented the sweating. The sweating, however, returned as freely as ever the following night, but on the four following nights her skin became dry, although this had never occurred previous to the employment of atropia. On the nights of Sept. 8th, 9th, and 10th she sweated as freely as ever. On the 11th, $\frac{1}{200}$ of a grain of atropia was injected, and her skin remained dry during the whole night. The injection made her sleep sounder.

On many occasions we injected $\frac{1}{200}$ of a grain under the skin of a woman suffering from advanced non-febrile phthisis, who sweated very freely on sleeping. On every occasion the atropia completely checked the sweating. Similar observations with $\frac{1}{200}$ of a grain, and with equally satisfactory result, were made on a man with febrile phthisis.

Our observations lead us to conclude that $\frac{1}{200}$ of a grain of atropia, injected hypodermically, is sufficient in most cases to check sweating for one night. Our observations are too few to determine whether after employing the injection on several nights the sweating can be relieved on discontinuing the treatment, but we believe that after a short course of this treatment the injection may be discontinued for a few nights without the return of sweating.

This treatment gave the phthisical patients better sleep, and we think allayed their cough; but unfortunately in most cases it caused very uncomfortable dryness of the throat.

Mr. Johnson assisted me in some experiments with stramonium. We found that, like belladonna, stramonium, subcutaneously injected, will very speedily check sweating, and produce dryness of the mouth. We noticed that while belladonna and stramonium checks sweating, they deeply flushed the face. Hence their influence over sweating cannot be due to their effect on the sympathetic ganglia thereby lessening the supply of blood to the skin, unless they can affect the blood-vessels supplying glands, while they leave unaffected the other vessels.

ON PUNCTURE OF THE BLADDER PERFORMED IN THE HYPOGASTRIC REGION BY MEANS OF THE CAPILLARY TROCAR AND PNEUMATIC ASPIRATION.

BY LÉON LABBÉ,

*Surgeon to the Hôpital la Pitié of Paris; Vice-Professor of the Paris School of
Medicine, &c.*

NOTWITHSTANDING the improvements which are each day added by surgeons to existing procedures of catheterism of the urethral canal, notwithstanding the most consummate individual skilfulness, yet there occur in practice a certain number of cases in which the natural passages do not permit us to penetrate into the bladder and to remove retention of urine.

Hypertrophy of the prostate, some very narrow strictures of the urethra, laceration of the canal caused by external traumatisms or by forcible catheterism, are the principal causes which bring on complete retention of urine, and give rise to indications for active surgical intervention bearing upon other situations than the normal passages.

All the various situations neighbouring the bladder have been utilised for the purpose of conducting into the bladder instruments capable of removing the retention of urine.

Perineal puncture has been abandoned by surgeons as being a most detestable operation.

Recto-vesical puncture, more or less advocated at various times by some surgeons, still enjoys a certain degree of favour; and quite recently, at the meeting of the British Medical Association at Birmingham, Professor Pemberton related the results of his practice on this point.

In France, M. Voillemier has employed, once only, *sub-pubic puncture*. The attempt was a successful one. But there is no doubt that this is again a procedure of somewhat difficult execution, and which does not seem to exclude the possibility of wounding some important organ, or the liability to urinary infiltration.

Lastly, *hypogastric puncture*, effected by means of large curved trocars, has been generally accepted by the surgeons of our country. A great many of them, however, think the procedure must be practised only as an extreme resource, and therein lies perhaps the cause of a great number of failures which have attended the use of the proceeding. It has been, besides, very diversely appreciated by surgeons, and that principally because many amongst them formed a definitive judgment without taking care to categorise the cases in which it had been employed. Now, an attentive investigation of all these facts has shown, as is well brought out by M. Pouliot in his inaugural thesis (*Ponction Vésicale Hypogastrique*, Thesis No. 110, Paris, 1868), that—

1. In healthy men, almost all young, puncture necessitated by traumatic causes is almost without danger.

2. Chances of success diminish in cases of stricture, because in these instances we find among the operated individuals who are exhausted by suffering, and reduced as it were to the condition of the aged.

3. Lastly, in senile affections, failure is about as frequent as success.

However it may be, and under all circumstances, hypogastric puncture practised by the aid of the old proceedings, even in an individual placed in the best conditions, constitutes an operation of some gravity, involving for the patient and the surgeon the necessity of minute precautions, with the object of maintaining the canula exactly *in situ*, under penalty of some complication ensuing, and that for a time which varies according as the flow of urine through the natural passages is more or less rapidly re-established. This canula, or the permanent sound which was substituted for it in a certain number of cases, could irritate or ulcerate the walls of the bladder as the organ returned to its normal size. Urinary infiltration, resulting from the passage of

the urine between the canula or sound when the bladder contracts, has been observed in some cases. Lastly, persistent hypogastric fistulæ, though they are rare, must be taken into account in appreciating the value of the proceeding.

It must also be noted that the operation of hypogastric puncture with the large trocars, though really simple, has often, through somewhat unfounded fears, been shirked by practitioners, who have thus lost the only chance of removing the grave, but often enough passing, accidents of retention of urine—accidents which, if not speedily and efficaciously combated, may give rise to the most serious complications, and often lead to a fatal issue.

It is a thing of habitual notice with surgeons who have had occasion to perform puncture of the bladder, by whatever proceeding it may be, that when once the bladder is emptied, and consequently the normal relations of this organ re-established, and when the tenesmus of the whole region is overcome, it becomes *generally* possible in a short time (a few hours, a day, two or three days) to re-establish the course of urine through the normal passages, whether the cause of the retention be due to traumatism of the urethra, hypertrophy of the prostate, false passages produced by the hand of the surgeon, or very narrow strictures of the canal.

The knowledge of these facts naturally encouraged surgeons to seek for an operative proceeding by means of which they might carry out the leading indication, namely, *to empty the abnormally distended bladder, and to gain time, so as to re-establish the natural passages without, if this be possible, causing the patient to run the risks of a serious operation.*

The day when M. Dieulafoy availed himself more completely than his predecessors of the notions which existed on the utility of capillary puncture on the one hand, and of previous vacuum applied to the aspiration of fluids on the other hand, and ingeniously combined these two means of action, he put surgeons in possession of a precious means of evacuating collections of liquids more or less deeply seated.

The capillary trocar alone, uncombined with previous aspiration, had sometimes permitted surgeons to make out the presence of a fluid in some situation of the body, and, after repeated

pressure, to empty the liquid collection more or less incompletely. But it was an uncertain means, varying in its effect according to a great number of conditions, and which, to produce some result, necessitated the employment of needles with a comparatively considerable calibre. Now, with the new apparatus which we have, we may make use of very fine needles (one-fourth or one-third of a millimetre), which separate the tissues without tearing them, and allow them when they are healthy to retract very speedily and completely. Notwithstanding this narrow calibre, through the aspirating action all liquids, even pus and mucus, are forcibly drawn into the instrument.

The first applications of the aspirating puncture to the evacuation of numerous fluid gatherings (pleuritic effusions, fluids in the joints, serous, sero-sanguineous, and purulent collections, &c.) were made as early as 1868 and 1869.

The idea of employing this operative means for emptying the abnormally distended bladder had necessarily occurred to the mind of many surgeons, when, in February 1870, the opportunity was afforded me of carrying out its application for the first time. The patient was a man of sixty-five, affected with hypertrophy of the prostate. A false passage had been the consequence of several unavailing attempts at catheterism carried on during about two days. The bladder was much distended, and the general condition of the patient was alarming. I determined to perform, in the hypogastric region and at two fingers' breadth from the pubis, a puncture with Dieulafoy's No. 2 capillary trocar. I by this means withdrew 500 grammes of urine, and the patient was immediately relieved. When the bladder had returned to its natural size, and the congestion brought on by this distension in the neighbouring parts had diminished, I was enabled to introduce a large sound through the urethra. From that time all accidents ceased, and the patient did well. The puncture had not given rise to the slightest accident or occasioned the least pain.

In the month of January 1871, I again had occasion to put into practice the procedure of hypogastric puncture by means of the capillary trocar and pneumatic aspiration.

The patient had, on the 4th of January, been thrown from a height of about two metres, and had fallen astride on a beam.

The result was a vast contusion of the perineum and of the abdominal region. The following morning the abdomen was extremely sore, and the perineum distended by a considerable sanguineous effusion. The patient had not passed water since the day before, and the dilated bladder extended as high as three fingers' breadth above the umbilicus. At the time the accident took place, about a large spoonful of blood had escaped through the urethra. Repeated attempts at catheterism were vainly made; first by the house surgeon, and afterwards by myself. An incision was made into the perineum with the object of affording an issue to the collection of blood, and of finding out, if possible, the posterior end of the ruptured urethra. A large number of clots were withdrawn, but, notwithstanding the most minute search, it was quite impossible to find a way for penetrating into the bladder.

Puncture with a No. 1 needle (Dieulafoy's apparatus) was made, and 400 grammes of urine drawn from the bladder. On January 6th the puncture was repeated three times the same day. The same was done on January 7th. On January 8th, the day of the patient's death, a last puncture was made, and the patient, who had constantly had fever, sank under peritonitis, the cause of which was confirmed by a post-mortem examination. Contusion and superficial laceration of the intestines and liver had brought on peritoneal inflammation, and the intestinal folds were strongly glued together. As to the bladder, it presented on its external surface slight ecchymoses, the size of which was not larger than a flea-bite. These little ecchymoses corresponded to each of the punctured points, but it was altogether impossible to detect any the slightest traces of the punctures on the internal surface of the urinary reservoir.

This case, with its post-mortem examination, showed the perfect innocuity of the punctures, even when repeated a great number of times.

On August 10th, 1871, my colleague, Dr. F. Guyon, surgeon to the Hôpital Necker, was led to perform hypogastric capillary puncture on a patient affected with numerous strictures, hypertrophy of the prostate, and laceration of the urethra, with a false passage. The operation was repeated

twenty-five times in twelve days, without the slightest accident occurring. The patient eventually died from the effects of dysentery, and on post-mortem examination it was stated that there existed on the internal surface of the bladder *four* little blackish spots corresponding to the capillary prickings, but without any infiltration or any loosening of the mucous membrane.

Since that time M. Guyon, M. Gosselin, surgeon to the Hôpital la Charité, M. René Blache, and M. Démarquay, have had occasion to employ, with success, hypogastric capillary puncture. In all the cases the puncture has permitted to overcome speedily all existing obstacles, and to transform cases which presented an exceptional gravity, into comparatively simple cases.

During this year, 1872, I have again had the opportunity of observing three more cases.

On December 31st, 1871, a patient suffering for a long time past from stricture of the urethra, and who could only pass water drop by drop, was suddenly affected with retention of urine. During twenty-four hours he made the most violent exertion to pass water. The efforts were such that pain became most dreadful, the rectum protruded, and formed an external cylinder of about ten centimetres length. The patient was brought in this condition to my wards on January 1st, this year. He was in a dreadful state of pain, uttering loud cries, and tossing his body about. The previous history of the patient, his present condition, the congestion resulting from the exertions made during twenty-four hours to pass water, necessarily induced the belief that it was a case of insurmountable stricture, at least momentarily. As I was convinced of the perfect innocuity of capillary hypogastric puncture, and of the *détente* relief which would follow the evacuation of the water accumulated in the bladder, I performed the operation, after having vainly tried to effect catheterism by means of the very finest bougies. Nearly 1,000 grammes of liquid were withdrawn. I was then able to reduce the rectum. The patient was put in a warm bath, where he was made to stay nearly two hours. He then expressed the greatest relief, and at that moment it was found possible to introduce a flexible bougie into the bladder. Catheterism was practised the

following days, and at a later period the operation of internal urethrotomy was performed.

In the course of the month of March a most robust male patient was brought into my ward at La Pitié, with infiltration of the scrotum and all the perineal region, and somewhat extensive gangrene of the same situation. By means of large incisions I combated the most urgent symptoms. Catheterism was then practised, but towards the fourth day I was informed during the day that the patient had not passed water for twenty-four hours, that the bladder was greatly distended, and that repeated efforts had been unsuccessfully attempted to introduce the small sound which had been in use the few previous days. At this time there existed an enormous abscess, in the midst of mortified tissues; an abscess, the opening of which, at a later period, caused the destruction of a notable portion of the inferior wall of the urethra. I attempted to sound the patient; and each time the end of the sound buried itself in the cavity of the abscess. I then immediately performed capillary hypogastric puncture, and withdrew 1,450 grammes of urine. The patient was relieved, and as soon as the bladder was emptied, and its normal connections re-established, it became possible to penetrate into the bladder.

The patient is now cured, and his urethral canal completely restored.

Lastly, towards the month of April, I was called at some distance from Paris to see an old clergyman, who was suffering from retention of urine, caused by congestion of a very hypertrophied prostate. Attempts at catheterism had produced a very extensive false passage; after which each new attempt at catheterism led the instruments into the lacerated portion of the canal. The patient had already had repeated shivering, his general condition was of the gravest, and it was with tears that he implored surgical intervention to put an end to his sufferings. Puncture was performed; relief immediately followed. Soon after, however, a new quantity of purulent urine gathered in the bladder. Another fit of pain, due to the distension of the bladder, ensued. Puncture was again performed. Attacks of shivering came on every day. Attempts at catheterism through the largely-torn canal were constantly unavailing. The patient seemed very pro-

bably condemned to die ; but it was important to spare him the atrocious pains which accompanied the retention of urine ; and during twelve days, the medical man who was attending him performed puncture three times a day, without ever any local accident occurring. The patient eventually sank from the effects of urinary infection, and of renal lesions which had long been in existence.

From all the foregoing facts the following conclusions may be drawn :—

1. That capillary hypogastric puncture is a perfectly harmless operation.

2. That in all cases it must be substituted for ordinary hypogastric puncture.

3. That in a great number of cases it may, when only once practised, allow the surgeon to penetrate afterwards into the bladder through the natural passages.

4. That in certain cases where catheterism is impossible, it may be performed three or four times a day without any injurious effect, and thus permit the surgeon to gain time and restore the natural passages ; and at the very least it constitutes a palliative means of the highest importance.

ON THE USE OF PANCREATIC EMULSION IN THE WASTING DISEASES OF CHILDREN.

BY DR. DOBELL,

Senior Physician to the Royal Hospital for Diseases of the Chest.

THE very interesting article by Dr. Prospero Sonsino, of Pisa, in the September number of the *Practitioner*, "On the Physiological Dyspepsia for Starchy Food in Infancy," revives an intention, from which I have been diverted by other occupations, of publishing a few words on an allied subject. In the annual announcements of the medical journals for 1871 my name was put down for an article "On the Use of Pancreatic Emulsion in *Tabes Mesenterica*," and, had I selected a less questionable term than *Tabes Mesenterica*, I should have fulfilled my intention of writing the article at that time; but when looking up my cases for the purpose, I found it so difficult to prove in those that had recovered that the mesenteric glands were the seat of disease, that I thought it better to postpone the article till I had a new series of cases in which the diagnostics of that point had been more minutely recorded.

In this paper I propose to drop the question of disease of the mesenteric glands, and simply to speak of the class of cases constituting that wretched form of "atrophy and debility" and "marasmus" in children, in which every part of the body wastes away except the abdomen; the state described by Dr. Druitt, in the last edition of his "*Vade Mecum*," in the following few and graphic words:—"Emaciation and voracity; the belly swelled and hard; the skin dry and harsh; the eyes red; the tongue strawberry-coloured; the breath foul; the stools clay-

coloured and offensive, sometimes costive, sometimes extremely relaxed; the patient usually dies hectic" (p. 75).

I wish to bring prominently forward the fact that this state, provided there is no advanced lung-disease, is rapidly cured by pancreatic emulsion given in doses of a teaspoonful every four hours, and regularly persisted in till fat and flesh are restored. It is, of course, necessary that a proper diet should be insisted on at the same time; but proper diet without the pancreatic emulsion will not do. This I have found over and over again in cases where everything judicious in the way of feeding and cod-oil had been carefully and perseveringly tried without avail, but which, on the addition of the emulsion to the previous diet, began at once to improve.

This fact has been familiar to me for a long time; and considering how largely pancreatic emulsion is now used in the wasting diseases of adults, I am surprised to find that it is not even referred to in the latest works on the diseases of children. Looking through these works and examining their indexes, one is led to the conclusion that their authors are not aware that there is such an organ as the pancreas, or that pancreatic juice has ever been used in any form in the treatment of disease.¹ Yet scarcely a week now passes but some general practitioner relates to me cases of the successful use in his own practice of pancreatic emulsion in the wasting of delicate children; showing that in this respect the rank and file of our professional army are in advance of some of their generals, which ought not to be the case.¹

Dr. Prospero Sonsino's paper will, I hope, excite more general attention to this important subject. He, however, has laid all the stress of his observations upon the influence of the salivary and pancreatic juices on the digestion of *starch*. This is unquestionably a point of the greatest importance in the case of very young children brought up by hand, as showing the absurdity of attempting to nourish them upon starchy food, not artificially digested, before the period of life at which the saliva and pancreatic juice attain their functional activity. And even then, as Dr. Sonsino afterwards remarks, "good reasons make us now

¹ The word "Pancreas" does not occur in the indexes of the last editions of West, Churchill, Vogel, Meigs and Pepper, Eustace Smith, Churchill, on Diseases of Children.

believe that really it is not proper to feed infants with copious starchy matters, however these may be rendered digestible." The principal results of Dr. Sonsino's investigations are summed up in the two following conclusions, which, however, are not new:—1. "Pancreatic juice in dogs, cats, and rabbits, in the first week of life—perhaps for some days more—is devoid of any digestive action on starch." 2. "In the early life of man, probably till the beginning of dentition, infants offer a true physiological dyspepsia for starchy aliments, caused by the inactivity of one at least—possibly of all—the humours that concur in the digestion of those aliments" (saliva, gastric juice, pancreatic juice, enteric juice).

No doubt, when wasting occurs in these early periods of life, it is very often due to foolish attempts to nourish children upon farinaceous foods, by which dyspepsia and diarrhœa add to the exhaustion of partial assimilative-starvation. But, as a matter of fact, farinaceous food is seldom depended upon without some addition of cow's milk or some assistance from lactation; and we see children suffer from wasting who are fed entirely upon cow's milk or nursed by their mothers, and in such cases the "physiological dyspepsia for starchy food" will not account for their decline. Therefore we must not forget, that although normal saliva only acts upon starch, normal pancreatic juice acts also upon fats; and it is probable that these two functions of the pancreas are sufficiently independent of each other that they may exist separately. This I pointed out in my paper to the Royal Society in 1868, "On the Special Action of the Pancreas on Fat and Starch" (Proc. Royal Soc. No. 97). It is there stated as the results of my experiments, that "in addition to the influence of the pancreas upon fat, it has the power of converting starch into glucose by simple mixture. This property remains to a certain extent *after the pancreas has exhausted its property of acting upon fat*. The quantity of pancreas which before mixture with fat will convert about eight parts of starch into glucose, after saturation with fat will still convert about two parts of starch into glucose." It is possible, therefore, that in different states of depraved health one or other of these properties of the pancreatic juice—that for the digestion of starch or that for the digestion of fat—may be deficient. And thus the

depraved nutrition due to such deficiency will not be limited to the period of life anterior to that at which, under normal conditions, the proper functions of the pancreas should be developed. It is evident that when the power of digesting fat fails to be developed at its proper time, the defect must tell with double force upon children already suffering from deficient digestion of starch.

The children who become the subjects of the kind of wasting of which I am now treating are especially (1) those who are suckled by mothers whose milk, though abundant in quantity, is extremely deficient in nutritive properties; (2) those who are brought up by hand; and (3) those who, at a later period of childhood, have been subjected to similar chronic defects in diet. Now, it is especially when the mother's milk is poor in fat and lactic acid that the child becomes "dissatisfied" and "craving," and in the majority of cases it is this which first leads to the introduction of farinaceous food, under the popular nursery belief that it is "*satisfying*;" and, as Dr. Sonsino states, if this is given before the power of digesting starch is established, of course nothing but mischief can result.

But organs, like individuals, do not rise to the full performance of their duties unless called upon by the necessity for their activity; and, as I pointed out in 1866 ("On Tuberculosis," p. 40, second edition), "As the mother is deprived of fat elements by lactation, so is the child deprived of them by a persistence in a diet deficient in milk. In the case of the child thus deprived of fat, a double injury is done—first, by cutting off the supply of fat elements necessary for the protection of the tissues; and secondly, *by paralysing the function of the pancreas by prolonged inactivity.*" I venture to think that this is a point deserving of far more attention than it has yet received. It accounts in a great measure for the impossibility of restoring these ill-nourished wasted children by any kind of *natural* diet after they have been allowed to remain in a chronic state of defective nutrition. A child that has been long fed upon diet deficient in fat fails to develop the fat-digesting properties of the pancreatic secretion, and thus, when proper food is at last presented, cannot make use of it for nutrition.

It is probable, therefore, that it is due to this conjunction of circumstances that these wretched cases of fatal infantile wasting occur;—the food deficient in fat not only fails to nourish the child, but fails to develop the function of the pancreas for the digestion of fat at a later period of life; the craving of the child due to the deficiency of assimilated fat is met by starchy food which it has not the power to digest, and which if digested cannot supply the place of fat. Thus it is literally starved from first to last of those elements of nutrition especially essential in early life. We cannot, therefore, be surprised that such cases have proved obstinately fatal, neither is it anything but what one might expect, *a priori*, that they get rapidly well when pancreatic emulsion of fat is added to their diet, for by this means they are enabled to assimilate both fat and starch.

Certainly, of all the satisfactory remedial effects of pancreatic emulsion, none equal the almost magical recoveries of some of these miserable wasted children. The cases in which I have seen it administered within the last eight years are too numerous to relate, and I will only briefly mention three of those which first especially excited my attention.

1. A poor woman came to the Royal Hospital for Diseases of the Chest with a child presenting the most exaggerated features of emaciation of every part, except the abdomen which was large and hard. She was very excited at having succeeded in gaining admission, and explained in great haste that “all she wanted was some pancreatic emulsion, which she had learned could be obtained at the Royal Hospital.” She said the child had been even worse than I saw it, that everybody told her it was a hopeless case, and that she had carried it to her mother’s home in the West of England, where it appeared to be slowly dying, when a charitable visitor came in and gave her a bottle of emulsion, saying that he had seen just such a case cured by it. She gave the emulsion, and the child began to improve so wonderfully that she was able to bring it back to London, where it continued to mend till her bottle of emulsion was finished, when it rapidly fell back and became nearly as bad as ever before she could find out where to procure more of the remedy. This she had just done, and hurried off to the hospital. The child had diarrhoea, but she said she knew that

would stop if I gave her emulsion, as it had done so before. I did as she asked—let her have as much emulsion as she wished, and the child got absolutely well. I have seen it this year, a well-grown, plump, hearty little girl. This woman has since had two other children, each of whom has in turn shown signs of marasmus like its elder sister; in one, when brought to me, the lungs presented small crepitation from end to end; but both of these children were put upon emulsion at an early stage of their wasting, and made easy recoveries.

2. Soon after these cases occurred, Dr. Dingley, of Argyll Square, consulted me about a little patient of his in Soho, who was wasting in the same way; and as all the usual remedies both in medicine and diet, including cod-oil, had quite failed to arrest the downward progress of the case, we agreed to try the pancreatic emulsion. I did not see the case again, but Dr. Dingley has since informed me that from the time of commencing the emulsion the child began to improve and steadily progressed till it got perfectly well; and it remains well to the present day. Dr. Dingley was so impressed with the success of the remedy in this apparently hopeless case, that he tells me he has since resorted to the same treatment in all similar cases with equally satisfactory results.

3. At the Oxford meeting of the British Medical Association, Dr. Langdon Down told me of a case that had made a great impression upon him, and it is especially important as coming from a man of his large and intimate experience in all that relates to the affections of childhood. The following note just received graphically indicates the outline of the case:—

“ 39, WELBECK STREET,
Sept. 14, 1872.

“ DEAR DR. DOBELL,

“The patient at Reigate was seen by me in consultation with Mr. Steele, in the spring of 1867. She was in the most attenuated condition I ever remember seeing. It appeared to be the extreme marasmus of mesenteric disease. The lungs were healthy. The treatment had been most judicious and exhaustive. As something which had not been tried, I suggested the pancreatic emulsion. The improvement was coincident with the altered treatment, and was very progressive. Five

months after I was asked to see her by her father, to test whether I could recognise her. She was playing croquet, and I could hardly believe that the one pointed out to me was our patient, the change was so great. She has ever since had excellent health.

“Yours sincerely,

“J. LANGDON DOWN.”

These cases, which are well known to many persons besides myself, may appear somewhat “sensational,” but they are only samples of numerous others which have occurred in my own practice. The fact is, that when these cases are properly selected for the treatment, they are all “sensational;” for the rapidity with which it takes effect, and the completeness of the restoration to health of children who appeared to be hopelessly dying, is simply startling.

I have proved over and over again that, whether in children or adults, no amount of milk or cream, however good, will do instead of pancreatic emulsion; and I have tried to discover why this should be. Milk, so far as this part of its composition is concerned, is simply an emulsion of fat; and pancreatic emulsion, as I have shown, in the paper to the Royal Society already referred to, is not, as formerly supposed, a chemical combination, but a true emulsion. Why, then, does not milk answer as well? I believe the explanation to be very simple, and that it turns upon the following points:—

1. The fineness of the particles of fat.
2. The permanent character of the molecular mixture of fat and water.
3. The proportion of fats having high melting points.

(a) In my first paper on Pancreatic Emulsion (*Lancet*, September 10, 1864), I gave the measurements (made by the late Mr. Farrants, president of the Microscopical Society) of the particles of fat in cod-oil and beef-fat emulsions, as then prepared for me; showing that the majority of the particles in the cod-oil emulsion ranged from the 16,000th to the 1,200th of an inch in diameter, and those in the beef-fat emulsion from the 10,000th to the 2,500th of an inch; and, according to Bowman (*Practical Handbook of Medical Chemistry*, p. 174), “The size of the

globules in healthy milk varies from a mere point to about the 2,000th of an inch."

Since I published Mr. Farrants' measurements, pancreatic emulsion has been made by a much more equal and satisfactory process than at that time, and I have just examined a chance specimen procured from Messrs. Savory and Moore, in which the large majority of the particles of fat range from the 21,600th to the 14,400th of an inch in diameter, the prevailing size being the 18,000th of an inch; while in a specimen of good new milk (cold), which I have also just examined, the large majority of the particles of fat range from the 7,200th to the 3,600th of an inch in diameter, the smallest being the 10,800th.

(b) The permanent character of the pancreatic emulsion is very remarkable, far exceeding that of milk. It "differs entirely from all other kinds of emulsion of fatty matter, whether chemical or mechanical. All other emulsions of fat are destroyed by ether, the fat being restored at once to its original condition. The influence exerted by the pancreas upon fats, therefore, appears to operate by breaking up the aggregation of the crystals of the fat. It alters the molecular condition of the fat, mingling it with water in such a way that even ether cannot separate the fat from the water. A *permanent emulsion* is thus formed ready to mix with a larger quantity of water whenever it may be added." (Proceedings of the Royal Society, already referred to.)

(c) In the *Chemical News*, September 4, 1868, I have stated my reasons for believing in the importance of fats of high melting points, such as stearine, margarine, and palmatine, over those of low melting points, such as olein, as elements of food and medicine; although further experiments and investigations are still needed on this interesting subject.

Pancreatic emulsion of solid fat, consisting principally of stearine, margarine, and palmatine, is therefore quite a different thing from milk, the fat of which is principally olein.

Now, the nearest approach to a pancreatic emulsion is what may be called *nascent milk*, by which I mean milk just secreted—milk that flows from the mammary gland as it is formed, or, as mothers term it, "as the draught comes in." In this the emulsification is finest and most perfect, but every minute that elapses after the milk is secreted deteriorates this perfection of

emulsification, until, as we know, whether retained in the lactiferous ducts or in an artificial vessel, but especially in the latter, and when allowed to cool, the cream separates from the water of the milk, never again to be susceptible of the same emulsification with water in which it first existed, *except under the influence of pancreatic juice.*

I submit that this is the secret of the superiority of lactation, and especially of lactation at the time "the draught comes in," over every other kind of infant-feeding, whether in man or in the lower animals. It forms an important distinction between milk-diet supplied by the natural process of suckling, and milk-diet administered artificially, and affords some reasonable colour to the old-standing belief in the efficacy of "new milk warm from the cow" for delicate children, and to the remarkable recoveries recorded in ancient times of old persons nourished by lactation when everything else had failed.

* * * The Author will be much obliged to any of his readers who will favour him with their clinical experience on the subject of this paper.

Reviews.

The Fallacies of Teetotalism ; or, the Duty of the Legislature in dealing with personal freedom, and an Elucidation of the dietetic and medicinal virtues of Alcoholic Liquors. By ROBERT WARD, Editor of the "North of England Advertiser." 8vo. London: Simpkin and Marshall. Newcastle-on-Tyne: R. Ward. 1872.

WE regard this book with very mixed feelings; regret at its publication, on the whole, greatly preponderating in our minds. It is, in parts, vigorous and clear, and might be very useful in confuting certain fallacies promulgated by the Teetotal party. But in proportion as we sympathise with the author's well-grounded contempt for shallow special pleading when employed by the Total Abstinence party, do we lament that his own zeal has betrayed him into very much the same kind of errors. Those who, like ourselves, are firm believers in the great utility of alcohol, both as medicine and as subsidiary food, but at the same time desire a rational and scientific test of its true place in the economy of life, can take no pleasure in the excesses of reaction, even against the dogmas which we most thoroughly disapprove.

Mr. Ward divides his subject under eight heads, with a view to establish eight leading propositions, which are as follows:—

1. That it is neither right nor politic for the State to interfere with the domestic practices or enjoyments of the people, except to the extent that such practices or enjoyments can be shown, in individual cases, after a fair trial, to be inimical to the general welfare.

2. That the use of alcoholic liquors is not *necessarily* inimical to the general welfare of society; and that, therefore, the excesses of individuals can furnish no legitimate pretence for interfering with the liberties of the whole.

3. That the present law¹ in regard to the sale of alcoholic liquors, whilst providing for the punishment of mischievous

¹ This was written before the passing of the recent Government Bill for regulating the liquor traffic.

excess, duly protects the sober use, and enjoyment of alcoholic beverages; and that it is not expedient to adopt a different principle of procedure in any future legislation that may be deemed desirable.

4. That it is a fair presumption, deducible from the teachings of history and the habits and customs of the various nations of the earth, that stimulants, whether alcoholic or otherwise, have acted an important part in the civilisation and intellectual development of mankind.

5. That it is a fair presumption, deducible from the fact that the Creator has endowed alcoholic liquors, in a striking manner, with the properties of being pleasant to the taste and agreeable to the stomach, that their moderate and reasonable use or enjoyment is essentially beneficial.

6. That there is reason to suppose that, except in particular cases, a forced abstinence from alcohol is exceedingly dangerous to mankind, both because of its immediate effect upon the mental and moral condition, and because the Creator has provided that where, from any perverse thought or idea, a reasonable and proper appetite has not been gratified, the result, sooner or later, is an overwhelming passion which defies control; and that, therefore, it is exceedingly probable that universal abstinence would terminate in universal drunkenness and ruin.

7. That the Maine Law, whether permissively or otherwise applied, would interfere with the private use of alcohol, would unnecessarily and unconstitutionally limit the liberty of the subject, would curtail or prevent the reasonable enjoyment of that which the Creator has made enjoyable, and would endanger the present progressive advancement and the future happiness of Great Britain.

8. That therefore, whilst never despising any truly temperate lesson or doctrine, by whomsoever taught, it is the bounden duty of every good citizen to oppose the introduction into Parliament of any bill containing provisions at all resembling those found in what is commonly known by the title of the Maine Law (or Permissive Bill).

With the first of these propositions it is hardly necessary to say that we fully agree. But the second proposition (although we entirely endorse its first position) seems quite illogical: for the "excesses of individuals" might have such an enormously detrimental effect on the general welfare, that it is very possible to conceive a state of things in which, for the general safety, it would be absolutely needful to restrain the liberty of every subject, with a view to render such disasters impossible. Mr. Ward must know as well as we do that this is only a matter of degree; that, for example, if alcohol could be proved to be as dangerous as strychnia, it would clearly be our duty to limit

the opportunities of buying it as jealously as we do (or ought to do!) in the case of the latter substance. It is quite possible that, even among non-medical persons, there are a few who understand the limits of the safe use of strychnia in minute doses, as a tonic; yet it is certain that if "strychnia bitters," for example, were allowed to be sold as a cordial, there would be wholesale accidental poisoning. Now, the teetotallers say that, considering the enormous range of its influence, alcohol, though not so immediately fatal to life as strychnia, is fully as dangerous to the general physical, and through that to the moral, status of the community at large. Of course we have chosen an extreme instance, and one which it is easy to prove not fairly comparable with the case of alcohol any more than is that of gunpowder, the restrictions on the sale of which were very unwisely put forward by the Teetotal party as an argument for similar restraints on the sale of drink: and Mr. Ward might, no doubt, turn the argument from the one into ridicule as successfully as he has turned that from the other. But it is after all a matter of degree, and we cannot assent to the dogmatic statement that the excesses of individuals can afford no legitimate pretence for interfering with the liberties of the whole. The true question is, Does alcohol sufficiently overpass the limits of those things which are chiefly foods or cordials and only rarely or accidentally poisons, and distinctly stand in the region of poisonous substances that are only occasionally beneficial to health? Of course we believe it does not; but we wish that Mr. Ward had confined himself more strictly to this issue.

In Proposition 3, Mr. Ward declares that it is not expedient to adopt any different principle from that of the law in force at the beginning of last parliamentary session; and in arguing against the adoption of the principle of the Maine Law, which he does for the most part very effectively, he introduces remarks that are far less defensible in our judgment. He will not admit that the existence of a multitude of public-houses is the cause of drink: he believes that it is the mere expression of our natural love of drink. We cannot stop to argue this point, but must express our belief that to a large extent this opinion is incorrect; and that, in fact, the unwarrantable multiplication of the centres of temptation to drink has a considerable share (though by no means so large as the teetotalers assert) in the spread of intemperance among the labouring classes.

It is, however, upon Mr. Ward's central theory of the causes of drunkenness that we chiefly desire to animadvert. Briefly, it is this:—That the desire for stimulants is an instinctive feeling in the human race, for the satisfying of which the Creator has provided alcohol and various other things of an analogous kind:

that the use of alcohol has had a large share in the civilisation of nations : that on the other hand, those peoples which have been deprived of its benefits, either from ignorance or from a perverse asceticism, have remained in a comparatively barbarous state ; and finally, that the result of such abstinence prolonged over many generations, is to induce a natural constitution in which the temptation to commit excesses with stimulants is enormous and overwhelming, and leads directly to brutish vice and national extinction, as in the case of the Red Indians, when once exposed to the temptations of " fire-water."

There is very much in this theory of Mr. Ward which strikes one as plausible, and if he had been content to apply it on a modest scale his arguments would have commanded our respect, though we might not have wholly agreed with him. That the moderate use of alcohol has contributed in an important degree to some of the best intellectual and physical work that the world has seen, we decidedly believe. Its remarkable union of force-producing with mere cordial effects, and its speedy digestibility, point it out as eminently qualified to play a useful part in the necessarily elaborate nutrition of highly civilised nations. It may even be true that in some degree alcoholic beverages have tended to the spread of civilisation itself. But the facts are capable of an obvious interpretation very different from that which our author puts upon them. That all highly civilised nations are found to be addicted to the use of stimulants may well be attributed to the far more complicated functions which their organisms, and especially their nervous systems, are called upon to perform, than those which savage races have any occasion for. And when, in support of the inverse view, Mr. Ward proceeds to such lengths as to state his belief that the extinct races of Central America which perished before the superior vigour of the Spanish invaders, and the Red Indians who are now becoming extinct, were the *descendants of the Rechabites of Scripture* (an eastward emigration having taken place), he not only transcends our belief, but must seriously impair the confidence of all impartial readers in the sobriety of his judgment. Indeed, his argumentation is often loose and wild in the extreme. For example, whereas the teetotalers have been justly charged with extreme want of candour in their attempts to strain Scripture to the sanction of their views, Mr. Ward really almost outdoes them in the irrelevancy and triviality of his quotations, both from the Sacred writings, and also from the least thoughtful (sometimes even the most perverted and licentious) writers who have ever praised the "genial" qualities of the juice of the grape. He is especially to be censured for his praise of those (really) intoxicating qualities of alcohol, by which knaves are occasionally made to display their knavery, and

worthy men to gush into maudlin outpourings of sentiment and affection. The sentiment of universal brotherhood and reconciliation which is experienced by soft-hearted and soft-headed men at a certain stage of vinous obscuration, is neither a trustworthy feeling in itself, nor can it be counted a proof of the good effects of alcohol; while the frequent repetition of such "genial" excesses is unquestionably likely to lead to something very different and much worse.

Upon one more idea of Mr. Ward's we must comment: wild as it looks, there really is a dash of truth in it. Dr. F. R. Lees, the Temperance champion (whom, in passing, we may describe as the most exquisite master of distorted reasoning who has ever come under our notice), has struck at least one vein of truth in reckoning among the more terrible consequences of alcoholic excess in the parent, the transmission of a degraded, nervous organisation to offspring, rendering them facile subjects of the temptations of drink. No fact in pathology is truer or more sad: the present writer has had frequent occasion to enforce its lessons. Well, says Mr. Ward, but then, most likely, the drunken parent only became such by rebellion of his organisation against the unnatural abstinence of water-drinking ancestors! One's first feeling, on reading this, is that of hesitation between the sense of its intense absurdity, and an indignant impatience with what looks like an attempt at frivolous quibbling on a terribly serious matter. We acquit Mr. Ward of the latter, however. We believe that, in a not very perspicuous manner, he has been impressed by a fact that is really of grave importance. That the inheritance of a nervous system virgin of all alcoholic influences renders a man liable to the temptations of drink, we certainly do not believe for one moment. But we are convinced that an ascetic disposition in a parent, leading him to proscribe not only alcohol, but *all sorts of innocent luxuries and amusements* for his children, has, in truth, very frequently driven the latter in manhood with fearful violence towards the undirected indulgence of every passion that they were obliged to bridle in youth. We believe it is this to which Mr. Ward unconsciously refers.

We are sorry that, in reviewing an author with some of whose purposes we heartily concur, we should have felt obliged to deal for the most part in hostile criticism. Let us say, in conclusion, that this is because the book is of most unequal merit; for there are parts of it which contain most useful facts and arguments, and the literary style is decidedly pleasing. Moreover, one cannot but feel that Mr. Ward has perhaps been tempted into rhetoric that his better judgment would disapprove by a generous indignation against the thick and thin partisans of the Alliance, with which we cannot but sympathise; and it

is with the keenest moral delight that one reads his outburst of scorn for the base persecution with which the Maine-league scribblers and agitators have visited poor Gough and others who, though firm adherents of the teetotal cause, have not been willing to go the extreme lengths of an intolerant faction. Were it only for that, we would wish to give him a hearty handshake at parting, and assure him of our sincere good-will.

Clinic of the Month.

Physiological Antagonism between Atropia and Morphia.

—Dr. Magee Finny related to the Medical Society of the Dublin College of Physicians an extremely interesting case at a late meeting. The patient was a young lady, aged 25, who suffered from neuralgia of the face, and Dr. Finny had previously found that she received much benefit from the subcutaneous injection of one-quarter of a grain of morphia and one-fiftieth of a grain of sulphate of atropine at bedtime. He therefore again went with the view of injecting this mixture, but noticing some fungoid bodies in the atropine solution, and thinking it was in consequence partially decomposed and weaker, he injected double the amount of the atropine solution. In the course of twenty minutes he was called to her, and found her cold and trembling, in a state of alarm, with a dry parched tongue and throat, great thirst, though she was hardly able to swallow drinks, with thick inarticulate speech, dilated pupils, and consequent impaired vision. Pulse 130. Respiration 32, and very shallow. The symptoms rapidly became more serious, noisy delirium supervening, when Dr. Finny remembered that morphia was a physiological antidote to atropine, and he forthwith injected one-third of a grain of the acetate of morphia. In five minutes the good effects of the latter were observable, the restlessness and jactitation ceasing, the pulse coming down, and the skin becoming warmer. In a few minutes the patient was asleep, and on the following morning woke free from neuralgia, and capable of eating a hearty breakfast. (*Dublin Journal of Medical Science*, No. vii., 1872.)

Colotomy for Intestinal Obstruction.—Mr. Steele commences by observing that many cases of intestinal obstruction terminate fatally without surgical interference, which, were timely operative measures adopted, would very probably end in recovery. He relates the case of a man, aged 52, who, usually enjoying good health, had lately suffered from diarrhoea. On June 2 he was unable to relieve his bowels; he took castor oil, but without effect. Mr. Steele saw him early next day, and found tympanitis, colicky pains, and faecal accumulation in the

rectum, with strong desire for defæcation. Various aperients and enemata were unavailing; the rectum was cleared out, and galvanism was applied, but without result. Bad symptoms soon set in, succeeded by failing power of the heart. This was relieved by ether and laudanum. Liquid food was well taken and retained. On the sixth day, the patient, who had somewhat rallied, suddenly becoming worse, colotomy was performed. Flatus immediately escaped, and fæces some few hours afterwards. Localised peritonitis, inflammation of the skin, diarrhœa, gastric and intestinal irritation, &c., gave great anxiety for about four weeks. By this time the wound was well healed round the intestine, and the patient improved, and became restored to fair health, but remained weak. No passage per rectum had since occurred; but free discharges of thick mucus had proved troublesome. A swelling high up in the pelvis, which before operation seemed like fæces, accumulated in the intestinal coils, afterwards descended, and proved to be a tumour, and the cause of obstruction. The patient was doing well. Mr. Steele concludes with observing that where the cause of obstruction is obscure, and appears to be fæcal accumulation, all legitimate endeavours should be made to dislodge the same; that when the cause of obstruction is clearly mechanical, opiate treatment should be immediately commenced, and operative interference promptly adopted; that in such a case as the one narrated, surgical aid is the only means of saving life; that a person with a tumour compressing the lower bowel is in a much better condition with an artificial anus than with a constantly forced passage by the natural orifice; that the growth of the tumour will not be nearly so rapid as if it were subject to compression by the fæces and strained defæcation; and that operation is most likely to be successful, when the obstruction is caused by tumour, there not being sloughing to fear, as in internal hernia, or intussusception. (*Medical Times and Gazette*, Aug. 24, 1872.)

Accident Splints.—Dr. Moffitt, of the Royal Victoria Hospital, Netley, observes, that whenever patients with fractured limbs, whether occurring in warfare or by accident, have to be transported from the place where the injuries have happened, it is most essential that splints of some kind should be applied before removal is effected. Splints, however, to answer such a purpose must differ in many respects from the permanent splints usually employed in the treatment of fractures; they should be light and portable, complete within themselves, capable of being rapidly applied, and so simple that bearers of sick and wounded, as well as the surgeon, can be taught to use them. Keeping in view these objects—namely, lightness and portability, completeness, rapidity of application, and simplicity—he has devised

two sets of splints, by means of which all fractures of the limbs may be temporarily put up. These have been recommended for use by the Committee appointed by the Secretary of State for War to report on the Hospital Transport of the British Army. The splints are in two sets; one set is for fractures of the upper, and the other set for fractures of the lower extremity. The set for the upper extremity consists of two pads, two straps, two buckles, two splints, and an arm-sling. The pads are made of strong ticking, each made in two pockets, one of which (the larger) is for containing the stuffing, and the other (the smaller) for sliding the splint into. One end of the pockets is shut and the other is open, but capable of being closed by a flap fastened with two tapes when the stuffing and splint have been introduced. On the outside of each pad, running from one end to the other, stitched at intervals, is a piece of narrow webbing. This is for attaching the straps to on one pad, and for loops to pass the straps through on the other. The straps are of strong webbing, each furnished with a brass buckle. They are fastened to the narrow strip of webbing on the outside of one of the pads in such a way that their position can be shifted, but that they cannot be altogether removed, thus obviating the likelihood of their being lost. The splints are Duncan's rattan splints, altered in dimensions and other respects to suit the purpose. Each is three inches and a half in breadth, one nine and the other twelve inches in length. The arm-sling is made of half a piece of calico a yard square, cut diagonally. The set for the lower extremity is of a very similar character. The weight of the complete double set is two pounds and a quarter. Though designed more especially for military purposes, he thinks they are well suited for use in mining and manufacturing districts. (*Lancet*, August 10, 1872.)

The Treatment of Puerperal Eclampsia.—Dr. Steele, of Liverpool, in a paper read before the Midwifery section of the British Medical Association, August 1872, justly remarks that there is no complication of labour which causes so much terror to bystanders, or demands greater calmness and self-possession on the part of the practitioner, or requires more prompt decision and discriminating treatment, than an attack of convulsions; and yet there are few of the accidents of childbirth, the appropriate management of which is the subject of such diverse and conflicting opinions. Until a comparatively recent period there was an almost universal agreement as to the necessity of free general depletion in all the same forms of this affection. Latterly, however, the necessity and even the propriety of the use of the lancet in this disease has been called in question. Chloroform, which undoubtedly has considerable control in many instances

over the paroxysms, has been considered by some to be a specific remedy, whilst others have advocated manual dilatation of the os uteri and speedy delivery; others, again, have found perforation of the membranes, and the evacuation of the liquor amnii, when that fluid has been in excess, sufficient to diminish and ultimately to arrest entirely the convulsions. It is probable that these differences of opinion are to a great extent apparent only, and that theoretical disagreement as to principles would frequently find a solution in a much greater uniformity in actual practice than might be supposed. The rule laid down by a large number of high authorities that copious blood-letting is the first and great remedy, the extent to which it is to be carried being regulated chiefly by the violence and frequency of the fits, is founded upon a twofold fallacy: first, an entire repudiation of the widely differing types and phases which the disease assumes; and secondly, a misapprehension of its true nature, and of the effects of blood-letting upon it. Convulsions have been attributed to an overloaded state of the cerebral vessels, as in apoplexy, to relieve which copious evacuation is necessary; when in truth, as was shown by Marshall Hall, eclampsia is a disease not of the brain at all, but of the true spinal system; and Trousseau and Niemeyer have also maintained that congestion of the brain in convulsions is an effect rather than a cause.

The propriety and extent of blood-letting must be estimated, not by the violence of the disease, but by the state of the circulation in the intervals of the paroxysms. In an overloaded condition of the vascular system, blood-letting acts as a direct sedative to the spinal cord; in the opposite condition, that of anæmia, or when carried too far, it only adds to the mischief, by increasing the already irritable condition of the spinal centres resulting from their anæmic condition; the pathology of the remedy, as Dr. Tyler Smith has quaintly said, thus closely trenching upon its therapeutic effect. There is, however, one condition in which blood-letting acts directly through its influence on the cerebral vessels; namely, when serous or sanguineous effusion, or over-distension of the brain itself, causes counter-pressure on the medulla oblongata, and thus indirectly gives rise to convulsions by spinal irritation. There is yet a further most important effect of blood-letting not generally recognised, namely, its preservative influence in lessening the turgidity of the vessels of the head, caused by recurring paroxysms, and which may lead to fatal cerebral congestion or effusion. Thus blood-letting in plethoric states of the circulation is curative in its action on the spinal marrow, preservative in its action upon the brain. There is a common belief that in the uræmic form of convulsions blood-letting is less effective, or altogether inadmissible, but this Dr. Steele thinks is a mistake.

Chloroform he regards as unquestionably a valuable remedy in convulsions; but it is neither a specific, nor can it supersede the necessity for blood-letting in plethoric conditions of the system. He does not believe that it will invariably lessen the violence or frequency of the paroxysms. These being truly reflex in their character, and the influence of chloroform over reflex action being limited after free depletion, or in cases where depletion is contra-indicated, it is most useful. Early and speedy delivery is undoubtedly the best treatment in certain conditions, namely, when by the eliminative process of reasoning we arrive at the conclusion that distension of the uterus, or the presence of the ovum, is the exciting cause of the fits, but the view that it should be adopted as a universal rule is shown to be a fallacy by the following facts. Convulsions in some cases do not occur until after delivery. This practice has been recommended on the ground that the uræmic condition speedily proves fatal first to the child, and then to the mother. But many cases are on record, where convulsions exist, where there is no evidence of uræmia at all; and again even when convulsions have lasted for many hours, the case has terminated favourably both to mother and child. Dr. Steele believes that the death of the foetus, when it occurs, is in most instances to be attributed to asphyxia from interruption of the placental circulation from the violence and frequency of the paroxysms, and not to uræmia. (*British Medical Journal*, August 31, 1872.)

Extracts from British and Foreign Journals.

Ætiology and Therapeutics of Miasmatic Pyæmia.—

M. Hüter divides pyæmic affections into two chief forms : pyæmia simplex, which is a pyæmic fever occasioned by direct or indirect infection with pure pus ; and pyæmia multiplex (metastatic pyæmia), occasioned by septic infection of thrombus, proceeding from the wound, and multiple collections of matter proceeding from the latter. The causes of P. multiplex are divisible into the indirect, or those favouring the extension and multiplication of thromboses ; and the direct, producing the softening or septic infection of thromboses. The production of thromboses is favoured by general and local disturbances of the circulation, producing lowering of the arterial pressure and prolongation of the stasis of the circulation throughout the whole nervous system, or in special veins : to the former belong old age, bad food, loss of blood, traumatic fever, intercurrent febrile diseases ; to the latter, local disturbances of nutrition, ligature, compressions of veins in consequence of inflammation. The softening of thrombi produces every kind of phlebitis. The septic infection of thrombi is partly chemical, and due to decomposing muscle and connective tissue ; and partly animal, due to monads (*microsporion septicum* of Klebs). The latter develop in large numbers in the form of very small round corpuscles in decomposing blood, pus, and urine, in which they live as long as the fluid contains oxygen. In putrifying muscle or connecting tissue water they speedily die, and are replaced by the larger staff-shaped nuclei. The monads produce locally diphtheritic inflammation and septic phlegmons. They enter the circulation in large numbers, producing erysipelas or diphtheritic inflammation of the subcutaneous connective tissue, septic infection of thrombi, and secondary pyæmic embolia. The vibrios only produce local putrefaction. They do not, on account of their size, and their relation to pure oxygen, gain entrance into the circulation. Pyæmia multiplex never arises from simple embolia, nor from infected thrombi, and they cannot therefore be characterised as embolia ; nevertheless, all infectious traumatic diseases do not depend on the entrance of fungi. Septicæmia originalis for the

most part, in intoxication with chemical putrid poisons, whilst the diphtheritic affections are produced by monads. Collections of matter may also be produced by mechanical or chemical lesions. Metastatic disease, especially of the lungs, very probably arises from the entrance of emboli permeated with monads, into the branches of the pulmonary artery, as well as by simple migration of monads (Klebs). The latter, perhaps, is the means by which the pyæmic inflammation of the joints and serous membranes arise. The treatment of metastatic pyæmia is, therefore, essentially prophylactic; the loss of blood in operations should be carefully avoided. The incisions should be clean and sure, and all contusions of the tissues by the ligature or *écraseur* should be avoided. The surface of the wound should be cleansed by irrigation with disinfecting fluid, as by Condry's liquid; carbolic acid dressing should be applied, and care should be taken that no secondary hæmorrhage occurs. The pus produced should be allowed to flow away freely by means of drainage tubes. Abscesses should be opened early, tight bandages removed, gangrenous tissues should be cut away, and diphtheritic surfaces should be cleansed by cauterisation, as by the actual cautery, or by concentrated carbolic acid; in erysipelas and lymphangitis inunctions of *pix liquida* may be employed, with steady pyæmic treatment of the fever and prevention of abscesses as far as practicable. (*Deutsche Zeitschrift für Chirurgie*, Band i., Heft 1.)

The Dangers of Operations on the Neck of the Uterus.—M. Alph. Leteinturier draws the following conclusions from his observations:—1. Operations (even when slight) upon the neck of the uterus may prove the *point de départ* of serious affections. 2. In such cases, more or less ancient lesion of the uterine annexes is discoverable. The operation seems, so to speak, to awaken the old inflammation. 3. Three circumstances may be referred to as favouring and explaining this:—1. A partial lymphangitis, commencing at, and spreading from, the neck of the uterus. 2. General congestion of the pelvic organs. 3. A congestion localised in some part of the genital system, occasioned by a reflex action originating in the cervix. (*Archives Générales de Médecine*, Sept. 1872.)

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¹ Any of the foreign works may be procured on application to Messrs. Dulau, of Soho Square, W.C.; Williams & Norgate, of Henrietta Street, Covent Garden, W.C.; or Baillière, of King William Street, Charing Cross.

THE PRACTITIONER.

NOVEMBER, 1872.

Original Communications.

TETANUS AND ITS TREATMENT.

BY C. MACNAMARA,

Surgeon to the Native and Ophthalmic Hospitals, Calcutta.

IN the *Practitioner* for September 1871 I gave the result of some eighty-three cases of tetanus treated by purgatives and Indian hemp, in the hospital under my charge. Of this number thirty-four patients recovered, an equal number died, and regarding the remaining fifteen cases we have no certain information. I then remarked that hydrate of chloral seemed to me "as an hypnotic to be invaluable" in the treatment of this disease, and in the worst cases—the temperature of the patient's body rising as high as 102° or 103° —that the extract of physostigma, as recommended by Dr. T. R. Fraser, should be administered in addition to chloral. Since writing the above I have had twenty patients suffering from tetanus in my wards: the details of these cases are published in the *Indian Medical Gazette* for January, June, and July, 1872; and I should wish therefore, on the present occasion, simply to note briefly the impressions gained from a further experience of the disease.

In June 1871 I adopted a plan of treatment in tetanus which I have since consistently followed, both in hospital and

private practice. It consists in administering forty grains of hydrate of chloral (to an adult) at bed-time, and in severe cases of the disease—the temperature of the body rising to upwards of 101° —an additional thirty grains of chloral is given at mid-day. The patient is made to swallow regularly every four hours about four ounces of milk, one egg being mixed with the milk, morning, noon, and evening; if the pulse indicates considerable weakness, beef-tea and brandy are substituted for the milk, but it is seldom necessary to administer food of this description. Milk and eggs, with arrow-root as the patient improves, is the diet which I almost uniformly order, it may be for twenty or twenty-five consecutive days. However serious the case may have seemed to be, a plan of treatment such as that described has been rigidly adhered to, the urgency of the symptoms not causing us to deviate from our attempts to procure the patient a sufficiency of food and sleep, and thus help him to live through the disease. By pursuing a plan of this kind I came to learn that tetanus (among the natives of this part of India) is by no means so formidable a complaint as it was generally supposed to be—it has undoubtedly a tendency, as we have long known, to run a course of some twenty or twenty-five days; and further, if we can only carry our patient through the first ten days of his illness, that, as a rule, a very favourable prognosis may be given of his recovery.

Of the twenty cases of tetanus above referred to, no less than seventeen have recovered under this treatment,—an unprecedented result, I believe, in the annals of the disease. These twenty cases were not picked; they were taken one after the other as they came into my wards, and constituted the entire number of patients, from June 1871 to June 1872, suffering from the disease in the Calcutta Native Hospital: some of them seemed in a desperate state when first admitted under my care. I still think that chloral has no power in diminishing the severity of the fits in tetanus, but it doubtless renders them less frequent, affording the patient rest and time to sleep; and this, together with a diet such as above noticed, has brought about the remarkable result of seventeen instances of recovery out of twenty cases of tetanus.

I continue to rely much on the thermometer as a guide to treatment in tetanus. I am aware that it occasionally appears to fail when the muscles of respiration are thrown into a state of almost continuous tetanic spasm, but as a rule it seems to me, as I stated in my former communication, that so long as the thermometer shows the temperature of the patient's body to be under 101° , we may content ourselves with giving the evening dose of chloral, however severe the tetanic fits may be. If the mercury rises beyond 101° there is danger ahead, and a second dose of chloral will be necessary in the daytime. Should the temperature of the body reach 103° , life is in imminent danger. After death from tetanus I have seen the mercury rise in the thermometer to 107° .

I am strongly opposed to the system of frequently repeated doses of chloral in tetanus, however violent the spasms, if the temperature of the body keeps below 101° : only one draught containing forty grains of this drug should be given; and I feel almost certain that, however bad the case may be, if an extra dose of thirty grains of chloral is administered during the day, we gain all the advantages in the treatment of tetanus that chloral can afford us. It seems that if the medicine be pressed further it is likely to endanger the patient's life, and a too prolonged sleep has appeared from time to time to be followed by a tetanic fit of such extreme violence as to kill the patient. It does not of course follow that because forty grains of chloral only puts a patient suffering from tetanus to sleep for a few hours we are to repeat the dose too soon; rather allow time for the drug to pass out of his system or become otherwise disposed of before again administering the medicine, otherwise the chloral or its derivatives may accumulate in the blood and ultimately poison the patient.

In fact, it appears to me that some cases of tetanus are hopeless from the first; the intensity of the spasms, especially of the muscles under control of nerves proceeding from the medulla, and the rapid rise in the temperature of the patient's body, being the earliest, and most prominent feature of the disease in dangerous cases: under these circumstances Calabar bean affords the patient the best, though a very poor hope of recovery. But a considerable number of cases will get well if

only treated according to the dictates of common sense. These patients should be kept in a cool room shaded from the light, and absolutely at rest. They must be fed regularly—a task frequently requiring a considerable amount of patience both on the part of the nurse and the sick person. Lastly, the patient should be put to sleep for some seven or eight hours in the twenty-four by means of the hydrate of chloral; and if the temperature of his body rises to $101^{\circ}5$ or to 102° , the sleeping draught must be repeated in the daytime, and an extra quantity of food administered in the shape of beef-tea and brandy.

LEFT SIDE HEMIPLEGIA OCCURRING DURING
ADMINISTRATION OF CHLOROFORM: DEATH
TWELVE HOURS SUBSEQUENTLY.

BY WILLIAM NEWMAN, M.D. LOND., F.R.C.S. ENG.

August 19, 1872.—I was asked by my friend and colleague Mr. Heward, of this town (Stamford), to give chloroform for him in a case in which he proposed amputation at the knee-joint. The necessity for the operation and the later condition of the patient he will kindly describe; I confine myself purposely to the chloroform administration, and to as clear a description as I can give of the appearances then noticed.

11.30 A.M.—I arrived at the house and was introduced to the patient's room: a stout lady, with unhealthy complexion, patches of dilated capillaries about the cheeks, and general pallor.

I listened to the heart's action: the sounds were regular, and very fairly good. Nothing there to excite alarm. Pulse 100, quick and excited beat.

Some stimulant had already been given: the patient lay back on the operating table, and at 11.44 I commenced to give chloroform. Skinner's mask was employed, and the chloroform shaken on this from a drop-bottle. She breathed quietly and easily; once or twice spoke of not smelling the chloroform. A few drops from time to time were shaken on the inhaler: there was no struggling or straining.

11.55.—Pulse had become more quiet, more steady and full, about 90. Respirations were about 30 a minute.

When eyelid was lifted, the conjunctiva was almost insensible

to the touch ; the left arm, when lifted, dropped at once by the side, and a pinch at the left wrist elicited no response.

Relying on these indications, I said that the operation might be begun : and at once Mr. Heward (to complete the diagnosis) introduced a trocar and canula into the mass of the tumour. The patient flinched, moaned slightly, and drew up the right leg. No movement of left leg or arm was noticed.

A little more chloroform was put on the mask, and the incisions for the anterior flap were commenced without eliciting any further expression of pain or consciousness. At this moment, as nearly as possible 12 A.M., she was sleeping quietly, without stertor and without consciousness of pain.

Very shortly afterwards I noticed the face to become a little dark, a shade of general lividity, so to speak, passed over it, and the respirations became more quick, 42 a minute, the pulse remaining perfectly steady and full at 84.

12.5.—I withdrew the chloroform entirely : the anterior flap had then been dissected up, and joint opened ; and I did not again give any anæsthetic, except for a moment or two later on.

The face was, almost at the moment of withdrawing the mask, covered with large drops of warm perspiration : this trickled very copiously off the face ; the body and left arm were equally wet.

12.25.—No sign of consciousness of pain. Blowing breathing occasionally for a few seconds. The operation now nearly over ; the flaps completed ; arteries tied ; and seeing the sutures about to be introduced, I held the mask for a moment upon the face. But she never flinched, and I withdrew at once the anæsthetic entirely. The eyeballs were now moving from side to side ; pulse 84 full, respiration 36, and an inarticulate sound was now and then to be heard from the lips.

She swallowed a little brandy and water with some difficulty.

12.35.—We moved her to her bed. She clasped firmly her right arm round my neck when asked to do so, and held firmly in the transit across the room.

12.40.—Is as yet semi-conscious only. I left the room to keep an appointment elsewhere.

Shortly after my leaving, I learn from Mr. Heward that he found the incoherent talking to continue ; she “ wanted to sit up,” &c., and on more close examination he discovered the left

arm and leg to be cold and motionless. From this condition she never rallied, and died at 12 P.M. same night.

Looking back on the case, I am inclined strongly to think that the fatal hemiplegia was of very early occurrence—probably even before the operation was begun; for though I found the left arm to drop heavily on being lifted, yet the puncture elicited expression of pain. Comparatively little chloroform had been used—none after 12.5—and yet the insensibility deepened, and remained marked (despite the absence of the anæsthetic) for the following twenty minutes.

On this reading, the intracranial hæmorrhage would be due more to the anxiety and fear of the operation than to any influence of the chloroform; and should be looked on as a coincidence, not as an effect of the inhalation.

The two other points of much interest seem to have been the unduly quickened breathing and the profuse perspiration, both indications of nervous disturbance, but not connected, so far as a tolerably wide experience goes, with the administration of chloroform. It does not occur to me to think of any precaution omitted, or of any other course of action which could for the better have influenced this very unhappy occurrence.

NOTES BY MR. HEWARD.

To make the history of the above case more complete, I add the following brief notes:—

Mrs. S. had had fairly good health up to her fifty-third year of age. On June 6, 1869, she was suddenly taken ill with *right* hemiplegia. The paralysis of the upper limb was complete, of the lower limb less so; speech much impaired; her mind remained clear, but she was dejected, and wept much. At the end of four months, recovery was apparently complete. In February of the following year (1870) she consulted me about a painful swelling on the fore and upper part of her right leg. She had noticed it some time, but had feared to see me, lest I should cut it. The tumour was about the size of a hen's egg, slightly elastic to the touch, firmly fixed, skin freely moving over it. I did not venture an opinion with respect to its nature until some time afterwards, when I came to the conclusion that it was a non-malignant tumour springing from the tibia. Soon I told the patient and her friends

that nothing could be done for cure, but amputation. To this she decidedly objected. As time went on the tumour increased; pain was excessive, both in the tumour itself, down the front of the leg, and in the foot. The extensor muscles of the foot became paralysed, and walking was difficult. In July of this year she went to London, and consulted an eminent surgeon there. He was of opinion that the tumour was non-malignant, most likely springing from the tibia, and advised amputation at the knee-joint. Still the patient objected to such an extreme measure. However, the tumour enlarging and threatening to burst the skin, and the pain increasing, she agreed to the operation.

August 19 was the day fixed upon. There was nothing worthy of remark in the operation:—A long anterior skin flap, a short posterior flap of all the tissues, the patella left *in situ*. The bleeding vessels were secured, and on inserting the suture needle into the flaps, it struck me as peculiar that there was *no* sensation of pain, although I saw that Dr. Newman had left off giving the chloroform. All dressing being finished, the patient was carried to bed. Dr. Newman and Mr. Gibbings, who had kindly assisted me in the operation, being obliged to leave, I remained with the patient. The effects of the chloroform passing off, I perceived that there was left hemiplegia; the patient was restless, moved the right arm and stump about, and wished to get up. For some time she could be roused by loud calling to; the breathing was irregular, and frequently stertorous. At 5 P.M. she became quite insensible, and at 12 midnight died.

Upon examination the tumour proved to be a complex cartilaginous one, apparently growing from the periosteum of the outer side of the tibia, near the upper end. About three inches of the surface of the bone was eroded. The tumour was ovoid, measuring six inches in its longest diameter. It had passed through the interosseous ligament to the back part of the tibia. It was very firm to the cut, and had many points of ossification in its structure.

[We cannot but observe that it is an immense pity that the brain was not carefully examined after death, though very likely this could not be managed. There seems to be great probability that *embolism* was the cause of each of the two attacks of hemiplegia.—ED. PRACT.]

GASTRALGIA AND TEA.

BY E. I. DIXON, L.R.C.P.

ATTENTION having been lately drawn to the pernicious effects of immoderate indulgence in tea, some information respecting its abuse in the busy manufacturing centres of Lancashire, such as Preston, may be perhaps not uninteresting; and as a typical example I will relate the last case which presented itself to me only two days ago.

A middle-aged married woman, who had previously suffered from no exhausting disease, but had enjoyed for the most part good health, complained, whilst feeling generally weak and exhausted, especially of pain in the epigastrium and umbilical regions, not of an acutely spasmodic, but of a severely aching character. This pain came on at irregular times, but principally a short interval after eating, and was, on some days, scarcely ever absent, except before breakfast. There was no abdominal nor spinal tenderness, nor were there any neuralgic foci; there was occasionally a slight degree of heartburn, but no nausea, vomiting, or other symptoms of dyspepsia. The tongue was clean, and somewhat pale, but there was no marked anæmia: bowels regular. No symptoms referable to disorder of other organs existed. Now for the cause of this disorder. I learned on investigation that the following was the system of diet:— Having to be at work at the cotton-spinning mill at six in the morning, she took before she left her house half-a-pint or more of hot tea, with perhaps a bit of bread and butter; at breakfast, a pint of hot tea and bread and butter; at dinner the same, with the addition of a bit of meat for the first day or two at the beginning of the week; at tea-time, the same; and at supper,

perhaps a glass of beer was substituted for the tea. On the Saturday evenings, having left work in the middle of the day, she would perhaps be able to eat a good meal with meat. On Sunday she would again live better; but by Tuesday, or, at the furthest, Wednesday, she had not the appetite, in the heated atmosphere of the mill, to take anything but her warm water meal. This woman thus subsisted, for the greater part of the week, upon bread and butter and hot infusion of tea, the quantity of the latter amounting to three and a half pints or more per diem. Now this is a type of a great number of similar cases I see every year in my dispensary practice; the successful treatment of which may be summed up in the substitution of milk for tea. Half-a-pint of good milk with a spoonful of rum in it, instead of the hot tea (often taken even without sugar), before going out to work; abundance of milk added to a small quantity of tea, at breakfast; and meat, if possible, for dinner, with beer if it can be taken. A bismuth mixture has also been prescribed, and a mustard poultice ordered to be applied to the epigastrium twice a week. When the patient appears before me the following week, he or she is invariably able to announce a material improvement; although in severe cases it has been necessary for the patient to leave work entirely for a time, for the purpose of rest, and in order to restore the appetite.

Now I do not assert that theine, or the warm water in which it is dissolved, is the cause of the gastralgic pain which these patients suffer; although I have seen cases which render it probable that even when a sufficient supply of really nutritious food is taken, the abundant ingestion of hot infusion of tea has a great tendency to impair the power of the stomach and produce indigestion; nor will I theorise upon the possible *modus operandi*. But it seems to me it is the substitution of a slightly stimulating but almost innutritious article of diet for others of more nutritive value which produces the general debility and the local pain. "A free breakfast table" has been set forth as one of the great points of freedom of taxation to be aimed at; but such cases as these I have briefly narrated may perhaps be quoted to show the undesirability of extending and promoting the consumption of tea.

CANNABIS INDICA IN THE TREATMENT OF MIGRAINE.

BY RICHARD GREENE, L.R.C.P. E.,

Assistant Medical Officer, Sussex Lunatic Asylum, Hayward's Heath.

ALTHOUGH the symptoms of migraine have been described with a clearness which forces us to believe that the writers must often have been themselves the sufferers, its treatment remains in a most unsatisfactory condition. The most opposite remedies have been tried and found useless, and at last the unhappy patient gives up all treatment in disgust, regarding himself as a martyr to a disease which time may enable him to outlive, but over which medicine possesses no control. Every physician has had strong evidence that this is too often true.

Arsenic, quinine, and other tonics and antiperiodics, together with subcutaneous morphia and alcoholic stimulants, seem to have the common property of being perfectly valueless as regards a permanent cure, seldom giving even temporary relief, and that in a limited number of cases only.

Some years ago I was induced to make a trial of the Indian hemp, and have since used it frequently, and have nearly always seen it productive of more or less benefit to the patient. The following six are the only cases of which I possess notes. They are consecutive, and have been attended within the last two years. Two only are patients in this asylum, convalescent as regards their mental affection; while the others are officials connected with the institution.

To guard against optimism, that common fault of medical writers, and source of our scepticism, I have detailed these cases as nearly as possible in the patients' own words.

CASE I.—Male. Had suffered from sick-headaches for about four years. At first the intervals of the attacks averaged at least fourteen days, but soon they were reduced to eight or nine, and latterly four or five days never passed over without a seizure. The Indian hemp was taken, though not so regularly as I wished, for eight weeks, and during that time the patient had but two attacks, and these were reduced both in length and intensity. It is now nearly three months since the use of the drug was discontinued, and in that time there have been two “very slight” attacks, and one rather severe, but of short duration.

CASE II.—Female. For upwards of twenty years this patient has been subject to migraine, and during the last few years the paroxysms, which were extremely severe, in fact the worst I ever saw, recurred every eighth or tenth day, and generally lasted from twenty-four to thirty-six hours, or even longer. Half-grain doses of the Indian hemp were ordered night and morning, and after having been persevered in for five weeks, great improvement followed; the attacks being very much slighter, and diminished in frequency to once a month. The dose was gradually increased to one grain, and now the headaches are still less severe and frequent, sometimes scarcely deserving the name of “sick-headache.”

Here, taking into consideration the duration of the disease, a cure could scarcely be hoped for; but after the drug had been taken for a few weeks a most marked amelioration was apparent. Moreover, this patient was an inveterate tea and coffee drinker, and could by no means be persuaded to give up the use of these wretched stimulants, otherwise there is little doubt that still greater relief might have been obtained. In this case there was vomiting with every paroxysm, and, as usual, the violence of the attack abated gradually after the stomach had ejected its contents. It might be argued from this that emetics would be useful, but direct experiment demonstrates that this is not often the result, and indeed can hardly be expected, when we remember that the vomiting merely indicates the lowest point of nervous depression.

Almost every kind of treatment had been tried with this patient; and although many of the *remedies* had been persevered

with for months and months, not the slightest benefit ever occurred.

CASE III.—Female. In this case the seizures recurred about five times a month, but the disease could scarcely be called severe, as the attacks were comparatively slight. They were reduced to once a month by one-third grain doses of the extract.

CASE IV.—Female. From the notes furnished me here, it would seem that the attacks were not diminished in frequency; but the patient admits not having taken the medicine regularly, though she states that a double dose taken when the headache was coming on often relieved it.

CASE V.—Female. Had been a sufferer for twelve years. The headaches had recurred about once a week, ten days being the longest interval. A month after commencing the Indian hemp she had the satisfaction of seeing the intervals lengthened to five or six weeks, and the attacks much slighter.

CASE VI.—Male. Here the seizures came on every fortnight or three weeks. The use of the drug was begun in half-grain doses upwards of three months since, and discontinued a fortnight ago. From the first dose being taken to the present time, this patient has never had a "sick-headache."

These will show that though the *Cannabis Indica* may often fail to cure, it scarcely ever fails to effect some improvement even in the most apparently hopeless cases; and Case II. will also show that this drug may be taken for very many months in comparatively large doses without producing any unpleasant effects or in any way injuriously affecting the economy. It would also seem that, unlike opium and some other narcotics, it may be given abruptly without requiring the exercise of any fortitude by the patient.

The best preparation is undoubtedly the alcoholic extract, and its purity cannot be too strongly insisted on, as inferior qualities lead to failure and disappointment.

The tincture is, I think, a faulty preparation, not only on account of its villanous taste, but because, after having been made for a short time, a deposit is frequently formed which may and probably does contain part of the active constituents, and is in danger of not being taken.

The dose should, of course, vary according to the age and constitution of the patient, and to the severity of the attacks. As a rule, it will be sufficient to prescribe one-third of a grain every night or every night and morning, and it may be increased to two-thirds of a grain. More than one grain should not be thought of except in very bad cases.

As an adjunct to the treatment, especially in anæmic patients, cod-liver oil will often be found highly useful, and the inhalation of three or four drops of the nitrite of amyl seems to be the most certain agent for giving temporary relief from the pain.

In the above cases, however, no drug whatever was used excepting the Cannabis Indica.

A CLINICAL LECTURE ON MIGRAINE.¹

Delivered at Westminster Hospital.

BY FRANCIS E. ANSTIE, M.D., F.R.C.P.

GENTLEMEN,—It is the just complaint of the physicians and the students of our hospitals, that the out-patient departments are always so overburdened with trivial cases that the attention of the observer is apt to be wearied out, and he fails to seize the points of the comparatively few really grave or otherwise interesting cases that present themselves. I think this circumstance makes it desirable that from time to time the assistant-physicians should deliver clinical lectures of a somewhat more formal kind than the casual talk which passes in the out-patient room, in order that the student may have the mass of rough material sifted for him, and be taught which are the important things to remember.

There is no disease which more pointedly illustrates this need for direction of the student's mind to the really important features of out-patient practice, than Migraine, or sick-headache. It is a common complaint in our out-patient room here, and it might readily be passed over as trivial and of slight import; but it is neither the one nor the other. It is a malady which possesses most interesting pathological relations with other and more formidable diseases; and it is very commonly the subject of gross mistakes in diagnosis and treatment, in consequence of the habitual ignoring by medical men of these, its true pathological affinities. It can at least be said, however, that in

¹ But for my desire to make at once the claim of independent observation, this lecture would have been absorbed into the course on "Nervous Diseases," now publishing in the *Lancet*.—F. E. A.

our hospital the true pathology of the disease has been taught for several years; and I consider it only fair to myself to point out this fact. More than one authority has lately put forward, as if it were a novelty, the doctrine that sick-headache is essentially a neurosis; that the stomach affection is a mere accident or secondary consequence of the nervous disturbance, and that the malady is frequently inherited. I could appeal to Westminster students of several years back to witness that, in the out-patient room, I repeatedly insisted on the importance of recognising the nervous origin of sick-headache, and pointed out both its position as an inherited neurosis, and also its special hereditary relations to certain other neuroses, particularly to epilepsy. Indeed, my article on Neuralgia in Reynolds' "System of Medicine," vol. ii. (written five years ago), states all these points quite clearly, my own opinion having been made up, and frequently expressed to our students, for some time previously. You will pardon this small digression, as I do not like that our school should be deprived of merit which it can justly claim with regard to priority in any matter of clinical observation, however small.

The first thing which you have to learn about sick-headache is its absolute distinction from the kind of headache which is the result of primary stomach disorder; so I will first briefly describe this latter, which is *not* the disease about which I mainly wish to speak. Headache is always more or less present in the catarrhal inflammations of the stomach which are among the commonest sources of indigestion. In the acute form of gastric catarrh the pain may be most intense and violent; but in such cases the disturbance of the stomach is so obviously the primary matter, and is attended with so much feverishness and general disturbance, that there is no danger of confounding the case with that of true sick-headache. In the true chronic catarrhal dyspepsias there is also headache from time to time; the pain is either frontal or (more properly) occipital in position, and is usually quite evenly bilateral, at any rate is rarely or never seated in one parietal region. The patient is generally strikingly languid, desponding, and unable to exert himself, even when he has no actual headache: he suffers from aching pains in the limbs, and very usually has a severe pain between the shoulder-blades. The tongue always has enlarged papillæ, most prominent

at the tip, and more or less thick furring, especially towards the back. The headache is often joined with nausea, but never with absolute vomiting unless there has been some special dietetic imprudence. The frame of mind is almost uniformly dull and heavy. Finally, the attacks are not at all periodic, but depend mainly upon accidental extra-irritation of the stomach by food; their intensity is proportional to the amount of previous stomach disturbance; they are not cured by sleep (as migraine almost invariably is cured); and there is no particular time of life at which they may not begin to trouble people.

Now see how very different from this is the picture of true sick-headache, in which I want to interest you to-day.

Migraine is a paroxysmal, intermittent affection, which, in the majority of cases, attacks people for the first time during some part of the period between puberty and the full bodily consolidation (of the bones, joints, &c.), which is completed about the age of twenty-five years. That is one remarkable fact, and there is another equally deserving your attention, viz., that scarcely any other form of neuralgia of the head, save migraine and the closely allied *clavus hystericus*, ever occurs during this part of life. So striking are these coincidences, that I ventured, so long ago as the year 1866,¹ to mark off a separate class of "neuralgias of the period of bodily development," represented almost exclusively by migraine and (in smaller proportion) clavus. Subsequent inquiry and the analysis of published cases have quite confirmed me in this view. I ask you, therefore, to fix in your minds the fact that *young* persons, though they may suffer from common headache the result of indigestion, or from pains in the head which are the symptoms of serious organic brain-diseases—such as cerebral abscess, tumour of the brain, tubercular meningitis, &c.—do *not* suffer from true paroxysmal *neuralgias* of the head, except in the form of migraine or of clavus. Let me say, now, the few words that are necessary about the latter affection, in order to clear the ground for the description of migraine. Clavus hystericus, so called, includes an ill-defined group of cases in which the pain is very intense, and limited to one or two small spots, usually in the temporal or parietal region of one side. They not unfrequently present

¹ In my Lettsomian Lectures on Neuralgias of the Fifth Nerve.

the same stomach complications which occur in ordinary migraine: and their only well-defined *distinctive* pathological antecedent (beyond the fact of their occurring by far most frequently in *women*) is the existence of special *poverty of blood*, either from actual hæmorrhage or from faulty blood-making, as in chlorotic anæmia. For practical purposes we may put this affection aside; and now let us inquire what are the clinical features of migraine, or true sick-headache, which is the ordinary neuralgia of the period of bodily development.

The subjects of first attacks of migraine are, in the majority of cases, slender and delicate young boys or girls, who may have plenty of vigour of disposition, but who suffer severely from fatigue after over-exertion, whether of body or mind. It is in one of these moments of temporary depression that the preliminary symptoms of an attack are observed; the tendency to it is much aggravated if *fasting* has been joined to fatigue. Usually, before the occurrence of actual pain, the patient feels uncommonly chilly and depressed, he may even be a little nauseated, but the tongue is clean, and it is evident this nausea is only one symptom of a general nervous depression. There are other warning symptoms, which may or may not occur: the same individuals are often found to have the same precursory symptoms before every attack. Thus some persons are always extremely giddy; others hear noises in the head; many others see flashes of fire, or globes of fire, before their eyes when they close them. A peculiar jagged light, resembling forked lightning, is a not uncommon appearance, and is terribly disagreeable to the patient. Many patients sigh, yawn, or shudder, or do all these things. At last the pain sets in; it seems for the most part like a series of dull heavy blows on the top or one side of the head; very often there is a deep fierce throbbing in the globe of one eye, and darts of pain run up to the vertex from the supra-orbital notch. If the pain much affects the eye or the forehead, there is pretty sure to be redness of the conjunctiva and a flow of tears after a little time.

A severe attack of migraine is a terrible affair. The pain, which at first may have been of moderate and irregular intensity, now assumes the form of regularly and rapidly recurring strokes of agony, in which one feels as if the head were being split

with an axe: the nausea, and the intense mental and physical dejection, constantly increase: the slightest sound or movement is perfectly intolerable.

I have now to mention a fact in the history of migraine which is of special interest and significance; it was first suggested to me as probable by my own case, and was afterwards established by inquiries on a large scale among out-patients and others. The fact is, that although in some patients migraine preserves its typical form during a long series of years, in the majority of cases in which the tendency to attacks of pain survives the youthful period the disease very distinctly alters its character. The alteration is in two particulars: in the first place, the stomach complication no longer occurs, or else is trifling; the patient no longer vomits at the crisis of the attacks: secondly, the pain loses more and more its deep-seated character, and assumes the ordinary type of neuralgia of the first, or ophthalmic, division of the fifth nerve.

Let me say that often some patience is needed to discover, in people who have outlived the migrainous period, that they ever had "sick-headache." But if you ask them whether they had "bilious attacks," they immediately tell you that they were very subject to that; and further inquiry will show that these bilious attacks were periodical, recurring every two, three, or four weeks, and that the vomiting was always preceded by some hours of intense headache, with sighing, yawning, or shuddering.

This leads me on to mention certain occasional complications of migraine by which its relationship to ordinary neuralgia of the ophthalmic division of the fifth is further illustrated. In several cases which presented the regular migraine type, there was observed either ulceration of the cornea, iritis, blanching of the hair, periostitis of the superciliary ridge, or some other of those affections of tissue-irritation which are well known to arise in a secondary manner in severe cases of trigeminal neuralgia. Some of you may remember a woman named Jordan, who was an out-patient here for some months in 1870: she was twenty years of age, and had suffered for a long time; the attacks occurred every fortnight, and lasted for many hours. A large part of the eyebrow and a thick lock of the hair, on the side where the pain was felt, had turned almost snowy white; the cornea was cloudy; and on the parietal eminence (which was

the seat of the most pain) the periosteum was so much thickened as to simulate a syphilitic node, but there was not the least evidence of syphilis, and iodide of potassium did no good whatever. Another patient, Martha Thomas, aged 17 when she first applied here, in whom the attacks came on monthly and had lasted for two years, had a persistent zone of sclerotic congestion, a *muddy* iris that had evidently been inflamed, and persistent anæsthesia of the skin of the nose and forehead. Here, too, there was the certainty that syphilis had never existed. The last time I saw this young woman her complaint was evidently losing the migrainous type; she still suffered from attacks of pain, but they occurred at uncertain intervals, and depended on temporary conditions of fatigue; moreover, they were hardly ever attended with vomiting. In my own case, while the affection was typically migrainous, there occurred, as complications, ulceration of the cornea, periostitis of the frontal bone, blanching of the eyebrow, and local anæsthesia (which last has remained permanent).

What are the pathology and etiology of migraine, then? In the first place, it is quite certain that the disease is a neuralgia; *i.e.* a periodically recurrent localised pain, the origin of which is within the nervous system, and is not dependent on any of the coarser and more obvious changes of structure which occur in organic diseases generally. The latest authority, Albert Eulenburg, admits that migraine is a neuralgia, but, oddly enough, denies that it is a neuralgia of the fifth nerve. Now, independently of the striking clinical relations which I have shown it to possess with ordinary trigeminal neuralgia, it would be difficult to say which nerve *could* be affected, if not the ophthalmic division of the fifth, or else the cervico-occipital, which, between them, practically furnish the whole sensory nerve-supply both to the exterior of the head and also to the cerebral membranes. Pain felt deeply within the head (if neuralgic at all), and neuralgic pain attacking any portion of the anterior half of the scalp, face, or eye, *must* belong to the fifth nerve. Migraine, then, is certainly a neuralgia, in the majority of cases affecting the first division of the fifth; in the rarer cases, in which the pain is at the back of the head, affecting the branches of the occipital.

(*To be concluded.*)

ON THE USE OF THE BATH THERMAL WATERS IN THE TREATMENT OF DISEASES OF THE SKIN.

BY JOHN KENT SPENDER, M.D. LOND.

Surgeon to the Mineral Water Hospital, Bath.

THE "medicinal stream," known as the Bath thermal waters, has had an immemorial reputation in the treatment of diseases of the skin. Used outwardly in the form of baths, and without any aid from external or internal medicine (properly so called), these waters have a soothing and "cleansing" effect, helping the depurating function of the skin-glands, and stripping away the dying or dead epidermic cells whose shortened life constitutes the essence of most chronic cutaneous disorders. True it is that this can be partially done by any sort of warm baths, *quoad* heat and moisture. But the mineral constituents of a "mineral water" modify its coarser physical qualities; and the influence of these are further wonderfully enhanced by mere bulk of water, and by the systematic employment of splashing, douching, and friction.

Many therapeutic elements, therefore, enter into the composition of a natural thermal water. It is not always possible to analyse and separate these elements in any rigorous way. Nature kindly offers us a gift almost illimitable in quantity, and our duty is to make the most of that gift by applying it far and wide, and finding out what it is good for by the simple process of experiment and trial. To treat any disease by water-dressing, water-packing, or even by soaking in the largest bath of domestic use, is essentially different from bathing in a quantity of water big enough to move about in or swim in: in this we have the friction of the warm fluid medium, the play of muscle, the

stimulus to the circulation, and the rousing of the function of sudoriparous glands. In these ways we may help many curative processes, and seldom are we able to do more manifest good than in improving the textural nutrition of the cutaneous surface. There is scarcely an organ of the body which has not been the battle-ground of solidism and humoralism, and the poor skin has been dreadfully vilified as the channel by which countless poisons have been supposed to make their way out of the body. There is very little evidence of the existence of any such poisons, and we shall take a more practical and logical view of the matter, by looking on most diseases of the skin as the simple results of tissue irritation or abnormal tissue growth. And so our immediate objects should be to soothe this irritation and to check this growth.

For general therapeutic purposes we may roughly divide all the common diseases of the skin into two groups: (1) a dry group, and (2) a moist group.

1. In the dry group of skin diseases, the genus Psoriasis (*Lepra alphos* of Erasmus Wilson) occupies the first and typical place. In the medical literature of the Bath waters (and very copious this literature was during the eighteenth century) there is a frequent record of the successful treatment of scaly eruptions of the skin. In the confused nomenclature of this subject which prevailed before Willan's time, we find a principal mention of two great classes of skin disorders—"scorbutic" and "leprous:" psoriasis and its congeners being the equivalent of the "leprous" kind, and the ekzemata representing the "scorbutic" kind. Now the preponderance of the former (or "leprous") class of cases is very remarkable. Dr. Oliver, a physician of repute in Bath a hundred years ago, gives the history of a number of such patients as they were admitted into the Bath Mineral Water Hospital: the symptoms are related with the preciseness of clinical detail so characteristic of the "old masters."¹

(a) In July 1758, a young man named Wingrove, living at Norton St. Philip, was received into the hospital. He was suffering from "leprosy," which means that he had a "branny

¹ Three Tracts on Bath Water. Tract the Third: "Histories of Hospital Cases under the care of the late Dr. Oliver." Edited by Dr. Charleton. Bath, 1774.

scurf, as white as snow;" it began on the right elbow, and spread to the "knees, hands, fingers, feet, and toes." Here and there the eruption began to "burst, chap, and bleed." After a preparatory course of "opening medicines," he duly bathed in the waters, and was discharged cured on January 10, 1759.

(b) Elizabeth Jordan, aged 16, having a "leprous complaint," was sent to the hospital in May 1758, by Dr. John Andree, physician to the London Hospital. She had a "white farinaceous crust," which began in "small red spots about the knees." The scales continually fell off, and were succeeded by fresh ones. They afterwards appeared on the "elbows, wrists, and the outside of arms and legs:" and the "crust itched intolerably." She bathed as usual after being bled and purged, and was dismissed well in January 1759.

(c) Benjamin Orford (no age given) was admitted presumably about the same year, afflicted with "leprosy." It began as a "red spot, as big as half-a-crown, on the right arm:" and there soon grew a "white, thick, chapped crust, which sometimes bled." It then came on the legs, and on several other parts of the body. By the usual treatment with the Bath waters, the crusts were washed off, but the redness and itching remained.

(d) Mary Clark, of Blaydon, aged 32, came into the hospital in 1774, suffering from "pustules, discharging thin glutinous matter;" and having "thin branny scales on one arm and both knees." She was an in-patient for 265 days, and was discharged cured.

(e) A poor girl (no name given) from Crewkerne, aged 18. Her complaint was "leprosy;" the "greatest part of her body and limbs were covered with large scabs, which scale off in white flakes." The duration of her successful treatment (17-46) was more than a year.

(f) Hannah Pew, from Sarum, aged 27 (no year given). She is said to have had "impetigo," or "dry furfuraceous scales." She was unable to "walk across the room without leaving some of these exuviae behind her." She was cured in 62 days, but some mercurial medicine is reported to have been administered.

(g) The last case I quote from Dr. Oliver is in some respects the most interesting of the whole. Mary Tomkins, aged 22, was suffering from "confirmed leprosy" all over her body. All

kinds of physic had been tried in vain, and she had been turned out incurable from St. Bartholomew's Hospital; "no person would take her into their house" (October 1763). Her state is suggested as resembling elephantiasis. The leprous scabs were rendered "soft and supple" by bathing; within eight months they were cleared away, and only dark-coloured marks were left on the skin. This person was bled and purged, and occasionally took alterative medicines; and an ointment made of tar and neat's-foot oil was applied to the skin.

An interesting summary is given by Dr. Oliver of cutaneous cases admitted into the hospital between May 1752 and May 1764. 241 lepers were received, of whom 122 were "perfectly cleansed;" 85 were much benefited; 12 were not benefited at all; 4 died; the treatment in 11 cases was discontinued because improper; 7 were discharged for irregularity. During the same period 50 persons were treated for "scorbutic eruptions;"—of this number 26 were cured, 18 much relieved, 1 died, and 5 were dismissed as "improper."

Two other old medical authors may be referred to. Dr. Guidott, writing in 1725, says of the Bath waters that their inward use is "very profitable in all foulness of the blood, the itch, scabs, leprosy, and the worms."¹ And Dr. Pierce, in his "Bath Memoirs," relates the "very great cures done upon leprous, scurvy, and scabby persons. The virtue of the waters is so well known in leprous cases, that it seems almost superfluous to bring many examples." Several instances are quoted of persons coming to Bath for the use of the waters towards the end of the seventeenth century.²

I have adduced enough to illustrate the traditional virtue ascribed to the thermal waters of Bath in the treatment of the "dry group" of skin diseases. A special value belongs to these observations in one respect, namely, that there was no "specific" treatment then in vogue at all. Most patients were once bled, and took purgative medicines at necessary intervals; so-called *alteratives* were sometimes administered, but it was seldom that drugs were prescribed for any length of time. Even the use

¹ "Discourse of Bath, and the Hot Waters there" (1725), p. 135.

² Quoted in Dr. Sutherland's "Attempts to Revive Antient Medical Doctrines" (1763), p. 289. This book is a real literary treasure.

of outward applications was the exception to the rule. It will be noticed that the management of the severer cases was very protracted, and might probably have been shortened by simultaneously resorting to different therapeutic means. The comparatively rapid success in other cases, however, may favourably vie with the results which we obtain at the present day.

Numerous persons suffering from cutaneous disease are still admitted into the Bath Mineral Hospital, and a large proportion of this disease is a variety of psoriasis, more or less pronounced or extensive. All ages and occupations are represented, and there is no marked predominance of sex. The medical history of most of these cases is pretty generally this: that every orthodox plan has been skilfully and patiently tried, including nearly always a prolonged administration of arsenic, and still a cure seems as far off as ever; or if there ever has been a complete disappearance of the eruption, it has returned in a little while with all its old obstinacy. The doctor is puzzled, the patient is discouraged, and finally the Bath waters are thought of as a pool of refuge. In the case of poor people, the hospital is a beneficent harbour for the reception of patients whose occupations are inimical to any steady treatment, or unfavourable to the prospects of permanent success. Better elements of hygiene are supplied, rest is enforced, and the skin is protected against those vicissitudes of temperature which injure its healthy functions. It is difficult to discover how much each of these factors contributes to the desired end; but it is fair to use them all as helps to that special treatment by water which it is the proper purpose of the hospital to carry out.

In the old days much importance was attached to "preparing" the patients for drinking and bathing in the thermal waters. We have simplified this innocent pedantry a good deal. The heart being proved to be sound in action and rhythm, and the other chief viscera seeming to be normal, the "leper" is allowed to bathe on alternate days, staying in the water up to his neck for a period varying from ten to twenty minutes, and never exceeding half-an-hour. The temperature of the water varies according as the patient is directed to have a "tepid" bath, from 95° to 98° Fahr., or a "full" bath, the heat of which may range to 110°. During his sojourn in the water, he rubs the affected parts of

the skin with his hands or with a flesh-brush, and strips off as much "exuviae" as he can. The whole cutaneous surface is red and glowing when the patient leaves his bath. Every precaution is taken against cold, and, as a rule, he is not allowed to go out in the open air afterwards on the same day.

It is a fact that a speedy and remarkable improvement usually occurs even in cases distinguished by the defiant title of *Psoriasis inveterata*. We notice that the thickening of the subcutaneous connective tissue, almost always the result of a long-standing scaly disorder, gradually subsides; and the accompanying irritation, which is often so troublesome, usually diminishes *pari passu*. Patches of clear skin become more marked, old spots become fainter, and the general health is strikingly improved. The question then continually recurs, Shall we give medicine or not? Now, the final cause of the Bath Mineral Water Hospital is not to try pleasing experiments with the waters, but to enable us to cure our patients as fast as we can. We constantly meet with cases in which medicine has been prescribed fruitlessly for weeks and even months before admission into the hospital, and the application of the thermal waters has caused such a change in the economy that medicine is resumed with distinctive benefit. Surely, then, the two may go together and work together. And further, I believe that the patient ought to be particularly instructed to continue his medicine for some time after he quits the hospital, if the object be to maintain the good already done. Among English dermatologists, the repute of arsenic seems to be somewhat losing ground,¹ and yet for no better reason, perhaps, than because it is not always administered in a judicious way. Hebra swears by arsenic still; and probably the whole truth is, that if we were generally to help its action by suitable external means, we should discover abundant ground for not withdrawing our faith in it. So I contend for the propriety of giving arsenic in a quiet steady way to most patients undergoing the water discipline, and in doses that never disturb the healthy functions. And though it is easy to diagnose the squamous syphilide, falsely called syphilitic psoriasis, yet there are plenty of examples of true psoriasis which have just enough of a dim syphilitic taint about them to justify the administration of that fine old

¹ Journal of Cutaneous Medicine, December 1870.

“alterative,” bichloride of mercury. It has been my creed for years that a combination of arsenic and bichloride of mercury is one of the most effective things in therapeutics.

I wish to draw the outlines of two cases which have a *demonstrative* quality about them, in reference to the point I am now discussing. The first occurred in private practice in the summer of 1870. A middle-aged maiden lady was troubled with *Psoriasis guttata* over nearly the whole body; there were faint silvery scales even on the face. The disease had existed for about three months, and had not yielded to domestic remedies. The general health was good; nor was there evidence of any special diathesis. A moderate arsenical treatment was carried out for several weeks with only small success. Next, the patient bathed twice a week in the Bath waters for four months, still continuing the same medicine, which never disagreed in any way. Her testimony was unequivocal as to the more rapid improvement during an equal portion of time; and as a crucial proof of the efficacy of this plan, the medicine was again taken for a time without the baths. Finally, the medicine was gradually left off, and the baths resumed at longer intervals. It was nearly a year before the disease was quite cured, but there has not been the slightest return of it up to the present time (October 1872).

The other case was that of a domestic servant (a cook), who applied to me last spring on account of an almost universal and protracted *Psoriasis diffusa*. She was under thirty years of age, of very short stature, but, except the skin disorder, there was no apparent derangement of health. The subcutaneous pigmentation was remarkably vivid, and the thin papery scales were dotted over it in an unusual manner. Medical treatment (external and internal) brought her up to a certain point of amelioration, when she was admitted into the Mineral Water Hospital, under the joint care of Dr. Hensley and myself. For a time she was permitted to bathe without any medicine, and the improvement was quite as rapid (perhaps more so) as when she took the medicine without the baths. After a month she took ʒj of the liquor hydrargyri bichloridi twice a day; and at the end of another fortnight, 4 minims of liquor arsenicalis were added to each dose of the mercurial solution. This patient was discharged

very nearly "cured" after a stay of three months in the hospital, and she was requested to continue the medicine for at least some weeks.

Now and then our hospital cases of obstinate psoriasis are treated with outward inunctions of tar; or a patient is enveloped in "tar sheets," and the baths are used only occasionally. Sometimes cod-liver oil is taken in small quantities for a long time with a very good effect.

2. The application of the Bath thermal waters to the moist group of skin diseases is not commonly attended with anything like the same success. Chronic ekzema is typical of this class, and there is abundant reason for believing that the protection and rest and better food afforded by a public charity may help to soothe and even cure a disorder which is inflammatory in its beginning. An ekzematous patient would most likely be allowed to bathe as a matter of course, for otherwise he could not be legitimately detained in the hospital; but it is the custom to use dusting powders and astringent ointments to a large extent, and therefore it is difficult to estimate the degree to which the mineral water contributes to the final result. And often tonic medicines are ordered too, if the general health seems to require them.

For the very chronic and drier varieties of ekzema, the diluted nitrate of mercury ointment may be usually relied upon. A still better application is the glycerine cerate of nitrate of mercury, recommended by Dr. Frazer in the *Journal of Cutaneous Medicine*.¹

On the general management of diseases of the skin, Dr. Hughes Bennett published some years ago a doctrine which may be put into a compendious formula.² Treat the scaly or dry eruptions (this was his advice) with the inunction of grease; treat the moist eruptions with the application of water. The moisture must be constant, and the vehicle of the moisture (lint well saturated in water) should be kept wet by covering it with oil-silk or gutta-percha sheeting. My therapeutic experience is

¹ Vol. i. 1868, p. 63. Dr. Frazer's paper, unluckily buried in a periodical which never enjoyed the circulation it deserved, should be read by medical men who have to do battle with obstinate ekzema.

² Clinical Lectures, 5th edition, pp. 837-839. Also the *Practitioner*, Oct. 1868.

curiously the reverse of Dr. Bennett's. The Bath waters (*quoad* heat and moisture, even if they have no further value) are proverbially efficacious for the scaly diseases, and we have seen that they can cure without the assistance of any other agent; and the common experience of every medical man tells him that the angry vesicular eruptions are wonderfully soothed by ointments of benzoated zinc or bismuth. If we treat an *ekzema rubrum*, for instance, with linen or lint soaked in any sort of lotion, it will be most disastrous to wrap this all over with an impervious covering; the morbid secretion is retained, and all secretions which are distinctly morbid decompose with preternatural rapidity, and become proportionately irritating. Moreover, a capital defect (as it seems to me) in Dr. Bennett's therapeutics of the skin, is that he assigns no place to glycerine. Now, glycerine has this property in common with the fats and oils, that it impedes oxidation; and so, when spread over an area of inflamed skin, these substances may stop the inflammation by preventing the access of its special fuel, oxygen.

The pustular and papular affections of the skin do not (I apprehend) come before us at the hospital with sufficient frequency to enable us to generalize the special success of the water treatment. Moreover, all purulent or puriform diseases of the skin imply something so wrong in constitutional strength or diathesis, that other more positive remedies must be brought into use; and the system might be injuriously weakened by warm baths. All papular or pruriginous eruptions are alleviated by water baths or vapour baths, which are greatly utilised for that purpose in private practice. Cases of contagious disease of the skin are not admitted into the Mineral Water Hospital.

The hydropathic method of treating psoriasis was invented and adopted by Priessnitz. The essential feature of the process consisted in what was called "wet packing;" and subsequently Priessnitz recommended the addition of douches and frictions. The plan was developed by Hebra into the continuous warm bath, the patient remaining uninterruptedly day and night in warm water;¹ but he admits that the effect has been merely such as he could have obtained by macerating the cuticle with lotions or

¹ Hebra "On Diseases of the Skin," New Syd. Soc. vol. ii. p. 34.

any other topical applications. Dr. Tilbury Fox¹ has recently revived the method of maceration by wet packing. In acute cases he advises alkaline and bran baths, with the inunction of oil; tracts of skin may be treated with water-dressing, and then covered with oil-silk; and when the scales are removed, greasy applications can be prescribed. This evidence must suffice to show the scope and limits of the treatment by water; but when I recall the fact that most patients bathe in the Bath waters only three times a week, and then for only twenty or thirty minutes at a time, there appears a reason for believing that some specific property must reside in these waters to explain the really remarkable benefit so often produced by them.

P.S.—I have omitted to say in the proper place that most patients under treatment for skin disease drink the waters as well as bathe in them. Adults in the hospital drink six fluid ounces daily before breakfast; children have four fluid ounces. We have had lately some remarkable cases of successful treatment of psoriasis in children.

¹ *Practitioner*, March 1871.

INFLATION OF THE LUNGS, AND HEART ACTION.

TRANSLATED BY ALEXANDER B. MACDOWALL.

IN a recent number of the *Sitzungsberichte* of the Vienna Academy, Dr. Hering discusses this question.

If a tube be fitted into the trachea of a dog, and air forced through it into the lungs so as to inflate them, and if the tube be then closed, a marked acceleration of the heart-beats occurs.

Dr. Hering, wishing to examine this effect more closely, operated as follows :—

He used a T-tube, inserting one end in the trachea of a dog, connecting another with a manometer, while the third could be used for inflation. The carotid or the crural artery was connected with another manometer, and the variations in both manometers were recorded by a cymographion. The dogs were narcotised with opium or morphine. When air was forced gradually into the lungs and the tube then closed, the first-named manometer rose and indicated the pressure on the inner surface of the lungs. As the animal tried to breathe, the mercury moved up and down ; if the pressure was considerable, so as to cause temporary tetanus of the expiratory muscles, the experiment was ended after the first deep inspiration.

Two effects were observed : the pressure of the blood fell in proportion to the tension of air in the lungs (doubtless owing to increased obstruction to its passage into the thorax and through the lung capillaries) ; also, the heart-beats were accelerated during the expansion of the lungs. In some cases, where the lungs were greatly inflated, the number of heart-beats was even tripled.

In these experiments the pressure (of air) was in most cases

not beyond 50 mm.; and Dr. Hering remarks that where the pressure is either very high or produced very suddenly, the effects are opposite to those described: so in some of Einbrodt's experiments, in which a vessel of compressed air was connected with the trachea, and the air allowed to enter suddenly on turning a cock, a pressure sometimes of 147 mm. was produced.

In seeking to explain the increased rapidity of the heart-beats in the above case, Dr. Hering first asks—Is it due to increased pressure on the external surface of the heart, just as pressure with the finger on a heart which has been laid bare will accelerate its action? It seemed *à priori* improbable that the gentle pressure employed, borne in great part by the expanded lung, could so stimulate the heart. But to test this theory, the thorax in some young animals was widely opened, and their lungs were inflated as in the former case. It was found that the heart-beats were accelerated, though in this case the pressure on the heart during inflation remained the same as before. In this experiment, of course, a much smaller inflating force sufficed for the desired expansion, the resistance being diminished. Some of the experiments were vitiated by the restlessness of the animal, but only those cases were taken in proof in which the animal was quite still.

Further experiment was made as to the possibility of pressure of a different kind producing the acceleration. A small perforation was made in the thorax and a tube carefully fitted into it, by which air was forced in; when this was done and the tube then stopped, acceleration of heart-beats took place in most cases; but the animals proved very restless, and the results were too uncertain, Dr. Hering thinks, to afford any proof. In another experiment he used the tube inserted in the thorax to exhaust the air, so that the collapsed lungs expanded somewhat again, when a distinct increase in the heart-beats took place. He considers it proved that increased pressure on the heart is not the essential cause of the acceleration of beats when the lungs are inflated, seeing that this acceleration takes place where the pressure is not increased, and even where it is diminished.

Might it then be due to the fact of obstructed circulation in the chest-cavity? It is perhaps conceivable that the alteration in the flow of blood might determine the heart to a different rate

of beating. This supposition, it is replied, must be laid aside, in view of the fact that the acceleration took place in the experiment in which the air was withdrawn out of the thorax; for in this case, the large vessels being under negative pressure, the flow of blood was favoured, and there was little increase of obstruction in the lungs through expansion. Further, the arterial blood-pressure was found in this case to have risen. Thus the conditions of circulation were altered in the opposite direction, and yet the effects of lung-inflation on the heart were the same. Direct experiment also showed that obstruction of the venous blood in the inflation experiments could not be the cause of the acceleration.

Dr. Hering next shows that the acceleration in question is not the result of altered conditions as regards exchange of gases. The inflation producing a greater pressure within the lungs, at first increases the passage of oxygen into the blood, while the escape of carbonic acid is small. Soon, however, the stagnating air in the lungs becomes poor in oxygen and rich in carbonic acid; while the blood flowing from the lungs is more and more venous, and the animal is afflicted with dyspnoea. Now, from the experiments of Thiry, Traube, and others, showing the exciting influence of a dyspnoëic condition of the arterial blood on the retarding fibres (*Hemmungsfasern*) of the heart, it might be expected that the dyspnoea produced by inflation would soon cause retardation of the heart's action; whereas acceleration occurs. It might be said that it is the great increase of oxygen received into the blood at the beginning of the experiment which causes this; the blood, thus more arterialised, removing the tonus of the heart-fibres. This, however, could hardly be the case, where, as often happens, the increase of heart-beats commences immediately with the inflation. But it is not always so.

To test whether the increased amount of oxygen was an essential factor in the case, Dr. Hering inflated the lungs with hydrogen instead of air, repeating the inflation several times, and after each inflation allowing the gas to escape by a branch tube, closing the latter before all the superfluous gas had escaped; inflating again, and so on. In this case, it was impossible the blood could become richer in oxygen through the inflation. But the acceleration of heart-beats took place as before.

Further, that the separation of carbonic acid from the blood is not favoured by inflation, either with hydrogen or air, is evident.

Once more, the supposition that dislocation of the heart may cause the acceleration, Dr. Hering also rejects, chiefly on the ground that this is scarcely a possible occurrence in those cases of moderate inflation which have been considered.

When both the vagus nerves are cut at the neck, the heart-beats are no longer accelerated on inflation of the lungs. Indeed, this section itself produces such an acceleration that an increase of the latter is hardly to be expected. But if, after the nerves are cut, the peripheral ends are then artificially excited so that the acceleration of the beats is reduced till they occur at the normal rate, and if the lungs are then inflated, no acceleration takes place in any case.

This co-operation of the central nervous system in the acceleration takes place, according to Dr. Hering, in the way of reflex action; the sensitive nerves of the lungs forming chiefly, of course, the centripetal path by which the change is transmitted to the cerebral centre, though the nerves of the diaphragm, the walls of the chest, and other parts may doubtless partake. The effect on the heart might be supposed to be either through excito-motor nerves or to be a withdrawal of the already existing excitation in the retarding fibres of the heart. Dr. Hering decides in favour of the latter view without absolutely excluding the other.

Reviews.

The Beginnings of Life : being some Account of the Nature, Modes of Origin, and Transformations of Lower Organisms. By H. CHARLTON BASTIAN, M.A., M.D., F.R.S., F.R.C.P., &c. &c. In Two Volumes, with numerous Illustrations. London : Macmillan and Co.

[FIRST NOTICE.]

THE task of a reviewer who encounters a book like that before us may be attempted in various ways, according to the critic's consciousness of this or that kind of fitness which he may possess for the undertaking. A large portion of the volume consists of arguments which must ultimately be assented to, or rejected by, the scientific world entirely upon questions of special microscopic observation, such as few—probably not half-a-dozen persons in this country—are in the least degree qualified to check and estimate. We at once disclaim any pretension to this sort of knowledge, and accordingly we shall not discuss the accuracy of Dr. Bastian's observations. Again, taking that view of the critic's duty which stands at the other extreme, we might content ourselves with a naked synopsis of Dr. Bastian's doctrines, in so far as they are novel; and on the whole, perhaps, it might be safest to rest content with this modest performance. We cannot quite reconcile ourselves, however, to this rather prosaic course; and accordingly we shall attempt, with what success remains to be seen, to convey something like an appreciation of the value of the biological arguments of Dr. Bastian (presuming his mere observations to be exact), and their bearing on that aspect of life-theories by which they come into most immediate relation with the prospects of therapeutic science.

The questions of microscopic examination and experimentation which, as we have said, cannot be discussed in this review, are pretty well known, in outline, to everyone. The dispute turns on the question—whether the experiments in which Dr. Bastian noticed the appearance of low organisms in fluids where no traces of them had previously been detected, were guarded by

sufficient precautions against the entry of living things from the air, or not. As regards this, though we have no means of practically checking the accuracy of Dr. Bastian's statements, we may decidedly say that to unbiassed outsiders it will seem that he has put M. Pasteur and the rest of his opponents in a very ugly and difficult dilemma. Certainly, a large portion of his criticisms of Pasteur are exceedingly damaging, nor do we at present see how they can be less than fatal, to the reputation of the latter as an experimenter of high rank. It is, of course, quite another question whether Bastian, in his turn, has exercised due care; we can only say that we perceive no flaw.

When, however, we approach Dr. Bastian's work from the point of view of persons who are merely possessed of an average acquaintance with current theories of life, we must at any rate acknowledge that it displays extraordinary ability, and deals with the most complicated problems with such perspicuity and such excellent literary art as to invest them with an interest which might captivate the attention even of the dullest student of biology. Of the chapters which compose Part I. (and which correspond to the "allgemeiner Theil" in German systematic works), we may state that there has been no such brilliant summary of the questions handled since the remarkable article of Dr. Maudsley in the *British and Foreign Medical Review* of October 1863; indeed, we cannot better express our high estimate of these chapters than by saying that they are of almost equal literary excellence with the latter paper, and present a more advanced view of the subject. It need hardly be said to readers of the *Practitioner*, that in rejecting all theories of life that imply the existence of any special vital force essentially different from the physical forces of the universe, Dr. Bastian has our entire sympathies: but it is not merely the general faith which he holds, but the unshrinking and logical clearness with which he carries out his principles, that charms us. We believe him to be right in saying that the present anomalous position of a few prominent physiologists—a position in which they manage to persuade themselves that the peculiar forms of force which are manifested in the animal organism are due to the guidance and supervision of a mysterious something which no mortal can reasonably distinguish from the Archæus of Helmont—is due to an unwarrantable shrinking from what they suppose is a career which must land them in the regions of atheism and materialism.

It is indeed strange, on the one hand, that men of scientific knowledge should see anything less wonderful, less worthy of an omnipotent and all-wise Creator, in the prescient design which could provide for the development of vital organisation by necessary evolution from its lowest forms, and on the other

hand that, in these days of high mental culture, men should be so scared by the mere word "materialism," that they fail to see the utter unreality of the old disputes about "matter" and "spirit," and their entire irrelevancy to the great problem of the origin and government of the world. Rightly understood, it seems to us that, whether true or false, the Darwinian doctrine of evolution, and the general tendency to trace uniformity and interchangeability between all the forces of the universe, most distinctly and directly tend to the exaltation, and not the degradation, of our ideas respecting the dependence of all things upon Creative Mind. The brunt of conflict which Dr. Bastian will have to sustain, however, is not with the dwindling sect of vitalistic biologists, but with the far larger and more influential section of scientific men who candidly acknowledge the substantial identity of the forces that originate life with those of the physical world, and yet cannot bring themselves to think it even possible that living things should originate from not-living matter. And here we may remark that Dr. Bastian has done very necessary work in clearing up a natural, but very unfortunate, confusion in the ideas of those who talk popularly about "spontaneous generation." He shows very well, first, that the word "spontaneous" is useless and unscientific; and secondly, that under that popular phrase two ideas are confounded, only one of which has (until now) been seriously advocated since the birth of scientific biology. No one, for a long time past, has ventured to revive the idea of Aristotle, that eels could be generated from mud, and so forth. The speculations of those who in recent centuries have advocated an origin of organisms *de novo* have not extended further than to the supposition of a possible heterogenesis—*i.e.* an origination of living beings from the fragmentary or decomposing tissues of other living creatures. That idea has indeed never been universally abandoned in any age: indeed, Bastian is able to show that even Harvey, whose dogma of *omne vivum ex ovo* has been made the great rallying cry of the whole army of those who deny a possible origin of life *de novo*, was at least quite undecided as to whether heterogenesis did not occur in some instances. The audacity of Bastian, however, carries him beyond the doctrine of heterogenesis: he believes in the possible and very actual occurrence of what he calls, for distinction, "Archebiosis;" that is, the origination of humble living organisms from not-living matter—*e.g.*, certain saline solutions. Unphilosophical as it may be, and little as we are prepared to dispute the justice of our author's caustic remarks on the inconsistency of those who reject vitalism and yet refuse to acknowledge the possibility of such a re-arrangement of not-living matter in such forms and under such conditions as admit of the development of life,

we must admit that this notion of Archebiosis took, and still takes, our breath away. Very stringent must be the proof, very multiplied and diverse must be the evidence, which could induce us seriously to believe that living organisms, however humble, can develop themselves from the elements of crystalline mineral matter. Once the first shock of the idea is got over, however, the reader will find far less to astonish him in the (very interesting) evidence produced by Bastian of the origination of such organism as *Bacteria* and *Torula* from not-living matter, than in the evidence which is brought forward (under the subject of Heterogenesis) to show that the lower forms of life are capable of development, and are still constantly developed, from each other, to an extent that no previous Darwinian philosopher has attempted to prove. Here, even more than in the chapters on Archebiosis, the reader, be he ever so candid, is obliged to draw his breath; still, we confess we cannot detect any error.

Upon all these matters, however, we propose to speak in more detail on a future occasion: we shall close our present notice with a few words to the *cui bono* inquirer, who may be disposed to ask what possible connection the question of the origin (or non-origin) of life *de novo*, and the development of higher forms of life from lower ones, can have to do with disease and its treatment, which is the special object of those who read the *Practitioner*. The connection, if not obvious, is very real and important. A very little reflection will teach us that if the question of the origin of life *de novo* is again to be seriously raised, there will come, closely treading on its heels, the question of the possible origination, *de novo*, or development by pathological heterogenesis, of many of those immensely important diseases—the zymotic class—which are now generally supposed never so to arise. The question has already reached this stage in Dr. Bastian's mind: we see this in one of his appendices, in which he shows that the rejection of Pasteur's views about natural fermentation for the broader physico-chemical theory of Liebig must inevitably bring about a scepticism as to all the evidence on which the so-called germ-theory of disease is built. It will be extremely interesting to recur to these views of Bastian hereafter, but especially to compare them with the alternative "graft-theory" of the origin of infectious diseases, to which we have very recently had occasion to call our readers' attention. Certain it is, that if we are to lay the foundation for anything more closely resembling a scientific prevention and treatment of these diseases than we can now boast of, these different views must again and again be sifted to the very bottom, as an indispensable preliminary of progress.

Clinic of the Month.

On the Treatment of Rheumatic Gout by the Aid of the Continuous Galvanic Current.—Dr. Althaus commenced some remarks on the pathology of rheumatic gout, by comparing it with the atheromatous process in arteries, or endo-arteritis deformans, and looking upon it as one of those highly insidious and chronic inflammatory conditions which are characteristic of the period of involution and senile decay. He said that the continuous current could not cure rheumatic gout, but it did a great deal of good in the following manner:—1. It acted as a general tonic to the system, and especially to the nervous system. 2. It promoted sleep, even where morphia and chloral had failed, or were inapplicable. 3. It relieved pain. 4. It reduced deformities. Dr. Althaus describes the mode of application in such cases, and laid stress upon the necessity of persevering with the galvanic treatment for some time. (*Medical Times and Gazette*, August 24, 1872.)

Tracheotomy in cases of Chronic Laryngeal Disease.—Dr. Eben Watson, of Glasgow, in a clinical lecture upon this subject, observes that most of these cases get better if properly treated without operation, but for this end long confinement and protracted medical care are required. Hence it is likely that a certain proportion of such cases will become worse from various causes not quite under the control of either doctor or patient. There is also a special danger in the tendency that commonly exists to acute exacerbations of the disease, and tracheotomy then becomes necessary. In proof of this Dr. Watson gives the details of two cases. The first case was one of long-continued chronic laryngitis, ending in ulceration of the glottis and necrosis of the right border of the thyroid cartilage. An exacerbation of the disease rendering breathing difficult, compelled Dr. Watson to perform tracheotomy. Chloroform acted badly, and Dr. Watson states that this has happened three times in his experience, the operation having to be performed whilst the patient is in a state of apparent death. All these three cases

recovered, and he recommends that the operation should be completed as rapidly as possible, artificial respiration being commenced as soon as the tube is fairly inserted into the trachea. Care should be taken to pull the tongue forward and jerk it frequently, a proceeding that he believes acts as a powerful stimulus to the pharyngeal branches of the pneumogastrics. A little brandy and water may also be given. The second case was one of acute exacerbation of long-continued laryngeal disease. Edema alone, however, was visible with the laryngoscope. In performing the operation, Dr. Watson thinks the isthmus of the thyroid gland to be of little importance, and that it is often cut through with perfect impunity, though if fairly seen it should be pushed up. The handle of the scalpel may be used to separate the parts in the middle line. A small sharp hook may then be plunged into the trachea to steady it. If the veins are very turgid and bleed freely, acupressure needles are very useful, for they are made to include and restrain bleeding from the entire lip of the wound; yet it is not necessary in urgent cases to wait till the hæmorrhage has completely ceased. (*Lancet*, August 3, 1872.)

Treatment of Croup.—In a lecture on croup, Dr. Robert Jordan, of Birmingham, observes that it was long before he could throw off the trammels of the old belief that the majority of the cases included under the name of croup have no false membrane formed at all, but are essentially inflammations of the mucous membrane of the larynx and trachea, accompanied by secretion of tenacious mucus, and also considerable swelling caused by effusion into their submucous areolar tissue; in other words, they are catarrhal inflammations of the larynx and trachea. When exudation is really present, it is a case of diphtheria. In croup there is fever from the commencement, with the peculiar dry barking cough that results from narrowing and dryness of the rima glottidis; and when once fully developed, the increase in the dyspnoea is rapid. After death the mucous membrane is found to be congested and swollen, and there is some tenacious mucus on its surface. In the treatment of such cases the child should at once be placed in a warm room (70° Fahr.) without draughts, the air of which is as far as possible saturated with watery vapour. A linseed-meal poultice to the throat is an important adjuvant. An emetic of ipecacuanha should then be given, and repeated every twenty minutes or half-an-hour, until not only copious vomiting, but copious perspiration, is induced. The cough becoming looser is an excellent symptom. In addition to the ipecacuanha, a very good prescription is a powder with calomel gr. $\frac{1}{4}$, compound ipecacuanha powder gr. $\frac{1}{4}$, and chlorate of potash gr. iij, every half-hour or

hour, according to the severity of the symptoms. In regard to the question of tracheotomy, Dr. Jordan would postpone its performance as long as can be done consistently with safety. If the case has been satisfactorily diagnosed to be diphtheria from the general feeling of illness, the absence of the peculiar cough, and the presence of the diphtheritic membrane or exudation, a different treatment should be adopted. Emetics, he thinks, are doubtful; the child should not be steamed, but the tinct. ferri muriatis, with the Haust. ammon. acet., be at once prescribed, and tracheotomy early resorted to: the strength should then be thoroughly supported. (*Medical Times and Gazette*, August 31, 1872.)

Yachting as a Resort for Invalids.—Dr. Charles Roberts, of the Victoria Hospital, London, points out the advantages of yachting for overworked and invalid patients. He divides yachtsmen into three classes: dilettante yachtsmen who keep a yacht as they keep a carriage, taking short cruises now and then in accordance with their own humour; true yachtsmen, who make their boat their home, and the sea their highway, and cruise about from port to port; and, lastly, the yacht-sailors, who enter into the higher and more exciting occupations of racing and making long voyages. The first kind has, he thinks, few advantages over a mere sea-side residence, but may be suitable for rheumatic and gouty patients; the second is the most suitable for invalids and convalescents; the last is fit only for the strong and healthy. Dr. Roberts notices the uniformity of the temperature in the cabin of a well-ventilated yacht; and observes that although upon deck there is little protection from the sun's rays, the air is always pure and the mental exhilaration is great. He considers yachting especially adapted for convalescents from surgical operations, and for those suffering from chronic strumous affections, and the secondary consequences of venereal diseases; and, above all, from overworked brain and nerves. On the other hand, the indifferent ventilation below deck and the dampness above, the necessarily irregular mode of living, as well as limited space for exercise, render it undesirable for persons labouring under diseases of the heart and lungs and abdominal viscera. It is the surgeon, not the physician, who should send his patients yachting. (*Ibid.*)

Notes on Chloral.—Dr. John Barclay records his experience of chloral hydrate in croup, insanity, delirium tremens, epilepsy, and asthma, and draws attention to a special peculiarity which he has observed with regard to its action when combined with iodide of potassium; namely, the remarkable power it possesses of intensifying the constitutional effect of the iodide. He gives

the details of a case of croup occurring in a child of fifteen months, which, after three days, seemed certain to die. Tracheotomy had been twice proposed, and the distress of breathing was extreme. More to relieve than with any hope of ultimate benefit, he ordered two grains of chloral with two minims of ipecacuanha wine, every two, three, or four hours, according to the effect produced. Ipecacuanha wine had been given before. Besides these, sherry was given freely. Before the chloral was given the respirations were from sixty to seventy per minute, and the pulse varying from 144 to 156, and weak. The dyspnoea was extreme, and the voice and cough now close and dry. Within two hours after the first dose great relief in the breathing was observed, the child slept for half-an-hour at a time, the respirations and the pulse fell a little, and, what was more curious, a very copious exudation of mucus began in the larynx and trachea; indeed, so great did this become, that it was necessary to raise the child every half-hour, when a little water or sherry was given to produce a cough by which the air-passages were cleared of mucus. Had the child been allowed to lie, it would soon have been suffocated; for after the chloral was given and the exudation had commenced, there was not the slightest inclination to cough. Gradually the pulse fell, and the respiration became slower, till at the end of twenty-four hours the former was only 108 and the latter 30. This treatment was persevered in for forty-eight hours, after which milk and sherry only were given. The recovery was slow, but steady. In regard to insanity, Dr. Barclay has treated seven cases in private practice: five were females, and two males. All were severe cases, and had insomnia for some weeks previous to the manifestation of delusions and illusions, and in none of them did the chloral fail to produce sleep. He agrees with Dr. Campbell, of Carlisle, in thinking that chloral in these cases is much to be preferred as an hypnotic to hyoseyanus, bromide of potassium, and even to morphia, as being a much more certain sleep-producer than either of these. Dr. Barclay thinks that it proves of little service in asthma, but he gives various cases showing its value in delirium tremens and epilepsy, and also a series in which, hydrate of chloral and iodide of potassium being given together in small quantities, no less than ten out of fifteen patients were affected with iodism by the first or second dose of the mixture. (*Lancet*, Sept. 21, 1872.)

The Treatment of Rheumatic Gout by Electricity.—Dr. Althaus regards rheumatic gout as a disease of malnutrition peculiar to the period of involution and senile decay, but observes that we must not be led away by the term “inflammation” into antiphlogistic measures, which only make matters

worse; but the great aim of treatment should be to support and stimulate the failing powers. The remedies upon which chief reliance is placed by the best observers are, he thinks, bark, mineral acids, strychnia, iron, cod-liver oil, and arsenic. That these remedies are of service we have the evidence of reliable practitioners; nevertheless, patients are often seen who have been under treatment for months or years and who yet find the distressing symptoms of their complaint gradually becoming intensified, the pain more severe, the deformity greater, the helplessness more embarrassing. Having had many opportunities of observing such cases, Dr Althaus has for the last six months sought and found a valuable remedy in that most active stimulant of nutrition, the continuous galvanic current; and although he is far from considering that this is a specific or panacea for all cases of rheumatic gout, yet he has seen enough of its effects to convince him that it may do great good in a variety of ways. The current cannot reproduce wasted cartilages nor restore its normal structure to a necrosed bone; it cannot prevent the patients from advancing in age, nor arrest altogether the process of involution and senile decay to which the system must in the end succumb; yet it can do much good. It acts as a general tonic to the system, and more especially to the nervous system, improving the digestion and the general actions of the chylipoietic viscera. The best mode of application for this purpose is to direct the positive pole, armed with a conductor of large surface, to the cervical spine, while the negative pole similarly armed is placed on the pit of the stomach. A gentle current, which even patients of extreme sensibility can well bear, is thus allowed to flow for from three to five minutes in the direction just described. By this application the base of the brain, the spinal cord, and the cœliac flexus of nerves are simultaneously brought under the influence of the current. This application is very effectual in relieving the symptom that is often so distressing—want of sleep. Pain, which is another invariable symptom of rheumatic gout, and which frequently resists the administration of sedatives and counter-irritants, is greatly relieved by the use of the constant current. It must in this instance be applied to the suffering parts, so that the positive pole, armed with a small electrode, is made to touch the sore points; while the negative, connected with a large electrode, is placed in the neighbourhood. The current used may in this instance be somewhat more powerful than where it is employed for general tonic effects; and if a short application, say of one or two minutes, be not successful, this may be extended to four or five minutes. The effect is, in many instances, almost magical, inasmuch as pain, which has been fixed in some particular spot for months and

years, is as it were charmed away by one or two applications. The deformities which rheumatic gout produces, more especially in the interphalangeal joints of the hands and feet, resist the use of the current more obstinately than the other symptoms which have been mentioned, but no one who has not seen it can have any idea of the beneficial effects which the current will produce in the long run, even when they are extensive, providing the patient is not very old. It seems as if the action of the vaso-motor nerves were gradually directed back into its proper channel. In order to reduce deformities, galvanisation of the cervical sympathetic must be had recourse to. In most of these cases a rapid cure is out of the question. The current should be used either daily or three or four times a week, for at least a month or six weeks, and, if the patient continue to improve, for even a longer period. General remedies should by no means be eschewed, and alkalies and bismuth, the Marienbäd or Friedrichshall waters, and steel, in the form of the ferrum redactum, or Spa water, should be prescribed. (*British Medical Journal*, Sept. 28, 1872.)

Treatment of Rupture of the Urinary Bladder.—Dr. Mason, of New York, at the conclusion of an article upon this subject, asks what are the resources which we may call to our aid. If the ordinary text-books on surgery be consulted, it might be inferred at once that, no matter what we do, death will most certainly ensue within a very few days at the farthest. Thus, Gross, in his work on the urinary organs, says: "It is obvious from what has been already stated, that no measures, however well directed, will in general be of any avail in saving life." The indications to be met are very apparent, viz., to prevent extravasation of urine and subdue inflammation. The stereotyped mode of accomplishing this is to prevent the patient from assuming the erect position or making any effort to void his urine. A flexible catheter, with an eye at the point as well as at the sides, is to be introduced, so that whatever urine may remain in the bladder may be removed, and either retained in it or frequently introduced. Should any sign of extravasation appear externally, free incisions are to be made to allow the escape of fluid and facilitate the casting off of sloughs; while inflammatory action is to be controlled either by calomel blisters, local blood-letting, or opium, the latter being now chiefly relied on. The frequent introduction of the catheter, or the confining of the instrument within the bladder for any length of time, is, however, often attended with serious inconvenience, and other plans of treatment have accordingly been suggested. Amongst them are:—1. Opening the bladder above the pubes as in the high operation for stone, or else simply tapping the organ in this locality. 2. Tapping the

cavity of the pelvis either above the pubes or the pelvic *cul de sac* through the rectum. 3. Perineal section, and then dilating the membranous portion of the urethra and neck of the bladder, as in the median operation for stone. 4. Opening the bladder either by the lateral or bilateral method, as in lithotomy. The last method, first practised by Dr. William Walker, of Boston, Dr. Mason considers to be the most practical and common-sense method of dealing with these cases that has yet been proposed or practised, and one which, more fully than any other, meets the most urgent requirements of the case, viz., the giving vent to extravasated urine, and preventing recurrence of the same, and this equally whether the rupture has taken place at the anterior or posterior portion of the bladder. (*New York Medical Journal*, August 1872.)

Ingrowing Nail.—Mr. Blower states that about twenty years ago he applied a bit of compressed sponge to afford temporary relief, and was delighted to find that it effected a radical cure. He makes the sponge as solid as leather by wetting and then winding string very tightly round it, and drying it thoroughly. Of this he cuts a small pyramidal piece less than a grain of rice, and inserts it beneath the nail, securing it by strips of adhesive plaster applied longitudinally to avoid compression. The sponge soon becomes moist and swollen, keeping the nail from the irritated flesh. Any granulations should previously be destroyed by strong nitric acid. Mr. Blower states that he has adopted this plan upon many occasions, and has never known it to fail. (*British Medical Journal*, Sept. 21, 1872.)

Feeding by the Nose in attempted Suicide by Starvation.—Dr. Anderson Moxey calls attention to the advantages of this method of feeding insane and other patients who decline to take food over the stomach-pump, which he believes to be the most unmerciful engine for the purpose of feeding that has ever been invented. The mode of conducting the process of feeding by the nose is first to see that the patient or prisoner has no weapon of offence, then to handcuff him or place him in a strait-waistcoat, or otherwise effectually control his movements. Three warders or more should firmly, strongly, but not violently, lay the man on his back on a bed, the pillows being removed. The patient's head should be received between the knees of the surgeon, the chin then elevated slightly, and a small smooth Wedgewood funnel introduced within, and only within, one of the nostrils, where it is to be held lightly and without pressure during the entire administration, it being borne in mind that it is used merely as a convenient medium for supplying the food to the nostril. Through the funnel thus held any fluid or semi-solid nutriment

may then be poured from a jug, taking care that it is done slowly and with occasional pauses, that the patient may take breath: for the reflex acts of deglutition, infallibly excited and vainly resisted, which follow the transmission of liquids through the nares into the pharynx, come so thick and fast that a rest now and then is necessary. In this mode Dr. Moxey can affirm from his own experience that from a pint to a quart may be administered in from five to ten minutes, and that the strongest beef-tea, wine, and even a pint at a time of the *Mistura spiritus vini gallici* of the British Pharmacopœia, with the addition of a strong dose of Liebig's extractum carnis, can be given without causing the eyes to water or the Schneiderian membrane to smart. After the administration of food, care should be taken not to leave the prisoner the use of his fingers to put into his throat and cause vomiting. The nasal feeding should be repeated every four hours till the disappointed and disgusted suicide entreats permission to feed in a more reasonable manner. (*Lancet*, Oct. 5, 1872.)

Extracts from British and Foreign Journals.

Etiology of Meningitis Tuberculosa.—Dr. Bierbaum terminates an elaborate paper which he has recently published, with the following conclusions:—1. There is an hereditary predisposition to this disease. It is seldom confined to one case in the same family; several of the children, as a rule, become subjects of it. The disposition may, however, be also acquired. 2. Such predisposition consists in the scrofulo-tubercular diathesis. There is no essential difference whether this diathesis makes itself known by altogether indubitable symptoms, or only shows itself by constitutional debility, which is nearly related to scrofulosis or tuberculosis, and leads to this disease. 3. It is nowise a necessary condition that the father or mother of the child should at an early period have been the subject of the disease. 4. The direct transmission of the scrofulo-tubercular diathesis from the parents to the children is of far greater etiological signification than the occurrence of the predisposition among collaterals. 5. Meningitis tuberculosa is peculiarly a disease of infancy, appearing far more frequently during the first than the second dentition period. 6. The sex exerts no especial influence on its frequency. 7. Whether the warm or cold season of the year most favours its occurrence is not decidedly settled. 8. Climatic, social, and anti-hygienic conditions are only so far operative as they influence the production of the scrofulo-tubercular diathesis. 9. This cerebral affection does not assume an epidemic character. 10. The exanthemata, and especially measles, are exciting causes of great importance. 11. Inflammatory affections of the respiratory organs more seldom give rise to this disease than pertussis. 12. In the abdomen lies a fertile source of many exciting conditions. (*Journal für Kinderkrankheiten*, 1871, No. 12, and *Medical Times and Gazette*, August 17, 1872.)

Treatment of Diphtheria.—Dr. Menzies states that he had recently the misfortune to witness an outbreak of diphtheria in an English family at Naples. The disease originated from drinking impure water. After discussing the mode in which

the contamination of the water was effected, and the symptoms that were produced, he continues:—The conclusions to be drawn from the above remarks are: That diphtheria, typhoid, and cerebro-spinal meningitis, have one common origin, and that place, time, and circumstance determine which of the three forms the disease shall assume. Dr. Menzies discusses the treatment he adopted in each case, and then proceeds to make the following remarks:—Free cauterisation at the outset, and repeated every day if requisite, is of the utmost benefit; a very strong solution of nitrate of silver is the best agent. Tincture of the perchloride of iron is the most reliable internal remedy; and he thinks it acts, in some measure, by the hydrochloric acid which it contains exercising a detergent influence on the diseased surfaces. Condry's fluid he has found of the utmost service as a gargle, and he thinks that it cannot be sufficiently praised. The patient should use it every hour, and the medical man should not neglect to wash his face in a solution diluted with water, and to gargle freely. He should also endeavour to fill the nares with the fluid. A strongly stimulant treatment should be adopted from the commencement. He has felt much inclined to try a preparation of strychnine dissolved in phosphoric acid (in combination with tincture of the perchloride of iron), which is much and successfully used by our American brethren in the treatment of malarious fevers. (*Edinburgh Med. Journal*, Sept. 1872.)

The Active Principles of Digitalis.—In a lecture reported in the *Bulletin de l'Académie de Médecine*, M. Rouher, *pharmacien principal de l'armée*, stated that from the examination of samples forwarded to him by MM. Gubler, Homolle, and Nativelle, he had arrived at the following conclusions:—1. That the green reaction of *digitaline* with hydrochloric acid belongs in general to most of the strong acids which act like hydrochloric acid. 2. That this coloration, which requires the contact of air in order that it should be produced, obtains also with other products of digitalis like *digitalcine* and the sublimed product of digitaline. 3. That digitaline can yield, by chemical change, glycose. 4. That by heat it furnishes a crystalline sublimate to which the author gives provisionally the name of *pyro-digitalic acid*. 5. That the homogeneity of crystallised digitaline is very doubtful. 6. That the globular digitaline of Homolle and Nativelle is capable of crystallising in three forms, and presents nearly the same reactions as crystallised digitaline, but that it contains another principle, *digitalose*, which has no action on the economy. 7. That *digitaline* give an amethystine violet reaction of great sensitiveness with hydrochloric acid, and when treated with acid changes into digitose. 8. That *digitaline* is the base of the greater number of soluble digitalines.

and that it is one of the most active principles of digitalis. 9. That the digitaline hitherto described as amorphous is capable of crystallising. 10. That digitalis contains two orders of agents, one but slightly soluble and acting slowly, the other very soluble and acting rapidly, both being important in a therapeutic point of view. Lastly, that as the green coloration with hydrochloric acid is common to digitaline, to the pyro-digitalic acid, and to digitaleine, a test for digitaline proper is still a desideratum. (*Bulletin de l'Académie de Médecine*, No. 21, 1872.)

The Treatment of Prolapsus Uteri without mechanical means.—Nicolai Andreef, of Kasan, states that he remarked that in some cases of disease of the uterus the application of tincture of iodine restored the relaxed and weakened ligaments to a nearly normal state. In consequence of this observation he determined to try its action in cases of complete descent and prolapse of the uterus. The first trials gave such favourable results that he was led to investigate its action systematically, and his results have proved so satisfactory that he now publishes them in the hope of inducing others to give the results of their experience with it. The following is one of his cases:—In August 1871, a patient who had suffered for four years from complete prolapsus uteri came under his care. She had tried numerous mechanical means for obtaining relief, without advantage. He prevailed upon her to submit herself to his treatment, and in four weeks he dismissed her cured. She was twenty-two years of age, emaciated, and of weak constitution. The prolapse had followed the birth of a child four years previously. The descent of the uterus was so easy that when replaced a strong cough brought it down to nearly its whole extent. The treatment pursued consisted in replacing the uterus whilst the woman was in a recumbent position. Then, with the aid of the speculum, the fold of the vagina, that is to say, the part surrounding the os, was painted with half a drachm of a tincture composed of one part of tincture of iodine and one part of alcohol. He diluted the officinal tincture of iodine because the undiluted tincture sometimes sets up acute catarrh of the vagina and even of the uterus; as he had had an opportunity of observing previously. After the application of the tincture the patient remained for three days in bed, and had an injection four times a day of pure spring water at a temperature of 20° R. The painting was then re-applied and the douches repeated. After a repetition of this plan of treatment four times the patient found herself well, and was dismissed. Four months subsequently she was pregnant and quite healthy; no descent of the uterus had occurred. He gives the details of other cases in

which the prolapse was both complete and incomplete. His experience has taught him that the best results are obtained when the following conditions exist:—1. The uterus must be capable of being replaced in position. 2. Before the iodine plan is adopted all other diseases and lesions of the vagina or uterus, such as erosions, ulcerations, &c., must as far as possible be removed, otherwise inflammatory reaction is apt to take place. 3. It is only requisite to paint the vault of the vagina, and at the commencement of the treatment, with dilute solutions and weak doses. Subsequently, however, stronger solutions may be employed. Cold vaginal douches must always be used after the application of the iodine with a view of preventing inflammation of the uterus and vagina. 4. In the majority of instances it is not requisite for the patient to remain in bed after the first two applications. After ten days she may be allowed to return to her work if not of too severe a character. 5. It is important that the bowels should be thoroughly cleared. 6. The interval between two successive paintings should not be less than three days; also, with the object of preventing irritation of the vagina and uterus, the cold douches may be continued for some time after the last application of the iodine. When the cure is complete, the vagina does not become narrower, but it is thicker than it was before. After reposition of the uterus, some sympathetic disorders of the stomach often occur, but this is easily remedied. (*Virchow's Archiv*, Band lv., 1872.)

The Treatment of Chronic Purulent Aural Catarrh.—Dr. Charles Inslee Pardee, of New York, observes first, that deafness of varying degree is almost an inevitable consequence of this disease; secondly, that weakened intellectual and physical powers are extremely probable if the disease be of long standing; and, thirdly, that meningitis, cerebral abscesses, uncontrollable hæmorrhage, and pyæmia are among the possible consequences. The product of morbid action, the secretion, indicates with great exactitude the character of the process, and can be always examined. According to pathologists, the changes, in a general way, are developed in this fashion:—First, there is increased vascularity, swelling, with a tendency to exudation and hypertrophy, and secretion which is mainly mucus. The mucous secretion is a characteristic symptom of the inflammatory stage of the disease, or of an hyperæmic condition of the membrane which lines the cavity of the tympanum. Secondly, these conditions may become modified in this way: the disease may go on to the development of excessive free-cell formation. The secretion then is purulent. The secretion, therefore, indicates the existing pathological condition, and enables us, with considerable certainty, to apply proper remedial agents. Now, the

precise object toward which our therapeutic efforts must tend seems pretty clear. They must tend to check the hyperæmia and set up a healthier action; to control and to limit the excessive development of free-cell formation. Essentially different applications are required in either case. All writers agree that thorough cleansing of the diseased surfaces is of great importance. It is absolutely essential. The effects of remedial agents are very uncertain unless brought in direct contact with the diseased parts. Owing to the irregular shape of the middle ear, and the existence (when it is diseased) of numerous little holes and crevices therein, the operation must be performed with the greatest care. The instruments required to effect the object are, an ear syringe, a Politzer's air-bag, and a cotton-holder. Syringe the ear carefully and slowly with warm water. The secretion in the external auditory canal will thus be removed. Then fill the ear full of warm water, push a piece of cotton into the meatus to keep the water in, and force air through the Eustachian tube and middle ear by means of Politzer's air-bag. The secretion in the tympanic cavity will be blown into the external auditory canal, and can be removed by another syringing. The operation may be repeated two or three times if necessary. Before applying remedial agents, use a soft piece of cotton on the cotton-holder and gently dry the parts. It must be done with the greatest care, otherwise too much irritation may be excited. The cotton does not seem to answer for cleansing purposes as recommended by some, but for drying after the parts are cleansed. Remedies are far more efficacious when applied immediately after the drying process. It is best to use remedial agents in a fluid form, and the easiest and most effectual manner of applying them is to incline the head of the patient to one side and then fill the auditory canal to the meatus. The fluid may be permitted to remain for two or three minutes, and then to run out. Applied in this manner it searches out every nook and cranny. The remedy, as we know, must be selected to accord with the existing pathological condition, as shown by the secretion. If the case gives evidence of hyperæmia, characterised by the secretion of mucus, use the nitrate of silver. In such cases this remedy is so effectual that a considerable number of them will recover without using any other. Instil into the ear a solution of forty grains to the ounce of water, and repeat the application every forty-eight hours until the discharge ceases or becomes purulent. In some cases it may be advisable to increase the strength to eighty grains to the ounce, after the weaker solution has been used continuously for a long time. It is an interesting clinical fact, that after the nitrate has been in contact with the parts for a minute or two, the entire effect of the application is secured, and may

be at once syringed out with warm water, thus preventing any stain about the meatus or auricle. Without this precaution unsightly stains are left. The appearance of a discharge of pus is by no means to be regarded as a discouraging circumstance. Indeed, as a matter of clinical experience, it is safe to assert that under the use of proper remedial agents we may confidently expect a gradual diminution in the quantity of secretion and a reasonably rapid recovery. But the treatment required is different. There is excessive development of free-cell formation, which, as we have remarked, is a process to be controlled and limited. The end is best attained by the use of weak astringents. Sulphate of zinc, acetate of lead and alum, are all valuable agents, and usually are quite reliable. Inasmuch as the desired effect is that of a weak astringent, other remedies which cause it may answer the purpose quite as well. Either of the above-mentioned remedies may be used in proportions of from two to four grains to the ounce of water, instilled into the ear in the same manner as recommended in the first class of cases. These remedial agents should be applied once a day, after a thorough cleansing. Of course, constitutional treatment, if required, must not be overlooked. Neither should there be any morbid condition of the pharynx. Attention to the general health and to the pharynx, in a large number of cases, is necessary. (*Medical Record*, Dec. 15, 1871.)

Method of removing Foreign Bodies from the Ear.—

Dr. Löwenberg remarks that as soon as the actual presence of a foreign body has been ascertained by ocular inspection, it is a point of great importance to determine whether the membrana tympani is or is not perforated. If this cannot be ascertained by inspection, he recommends that the auditory meatus should be filled with lukewarm water, by cautiously dropping it into the meatus, and then making a powerful effort of respiration with closed nostrils, or driving air into the tympanic cavity by Politzer's method; in both cases air bubbles through the fluid.

If the tympanic cavity be not perforated, and if the foreign body be small, and easily moveable, injections of lukewarm water will often effect its removal. The mode in which he thinks this acts is by the rebound from the tense membrane. Hence the stream should not be directed upon the foreign body itself, but as much as possible above. The head should at the same time be placed as far as possible in such a position as to make an inclined plane, and thus permit the foreign body to roll out.

If, however, the membrana tympani be perforated, either by the foreign body or by attempts to remove it; or if the body completely fills up the meatus; or lastly, if the injection plan

have been tried ineffectually, he recommends the trial of his agglutination or adhesive method. This consists in preparing a little brush by winding a strip of linen round a small wooden rod, and fraying out the extremity for a short distance. The patient is then placed with his head reclining on the head of a sofa, and the extremity of the brush, dipped in a concentrated solution of gum, is allowed to rest against the foreign body. After about three-quarters of an hour, the two will be found to be firmly adherent, that the body may be readily withdrawn. The part of the foreign body to which the gum is to adhere must be dry. Difficulties arise in this respect, if there be any discharge, but these are not for the most part insuperable: where gum is ineffectual, anhydrite or cement may be employed. The adhesive method, he thinks, might be employed even where the foreign body is contained in the cavity of the tympanum, provided the perforation be large enough to admit the brush. He communicates several cases in which he practised this plan with success. (*Berlin. Wochenschrift*, 1872.)

Cold Affusion as a Therapeutic Agent.—Dr. Lambert, in the *Journal de Médecine de Bruxelles*, thus summarises the indications:—1st. That it is especially useful in typhoid fever and the exanthemata. 2nd. It acts upon the principal and most constant symptom of these diseases, the elevation of the temperature, which it reduces several degrees, being especially a pyretic. 3rd. It favours the re-establishment of a full and regular respiration. 4th. It increases by reflex action the peripheral circulation by the rhythmical and vigorous contraction of the capillary vessels. 5th. It increases all of the physiological secretions, and renders the skin moist, soft, and cool. 6th. It generally favours the appearance of the rash, and re-establishes it if it has disappeared. 7th. It quiets cerebral excitement by stimulating the circulation of the brain, and thus diminishes delirium, coma, and prostration. 8th. It produces a tranquillity which allows the patient to sleep well. 9th. It diminishes the frequency of the pulse by from 8 to 30 pulsations. 10th. It cures the headache. 11th. The apyretic action lasts from two to eight hours. 12th. The affusions should, upon the average, be repeated from two to four times a day. 13th. It is especially indicated in severe typhoid or the malignant form of eruptive fevers. 14th. It does not shorten the duration of these diseases, but it diminishes their gravity, and renders them milder. 15th. It is not indicated in every case of typhoid fever, or of the eruptive fevers, so that it must not be regarded as an exclusive method without concomitant treatment. 16th. It is advantageously employed either by cold envelopment of the trunk, or by cold affusion practised simultaneously. 17th. It is easy of applica-

tion, and not disagreeable to patients. 18th. Its employment is rational, and based upon the teachings of clinical physiology.

The same author then gives the indications and contra-indications to this mode of treatment in typhoid fever. 1st. When the temperature exceeds and remains above 39.5° Cent. (103° F.), rather than in the usual grave type. 2nd. When there are grave nervous phenomena, such as furious delirium, *subsultus tendinum*, violent agitation, coma, insensibility, or stupor. 3rd. When the respiration is irregular or insufficient. 4th. When there are from 130 to 140 pulsations per minute, and when they are feeble and regular. 5th. *When the skin is dry and parched.*

The following are the contra-indications:—1st. Intestinal hæmorrhage; since all authors, with the exception of Brand, have observed an augmentation or a more frequent repetition of this grave accident after its use. 2nd. Intestinal perforations; as in these cases the least movement is painful to the patients.

The valuable observations of Prof. Béhier warrant the employment, in these cases, of a continuous application upon the abdomen of bladders of ice. The following conditions do not contra-indicate:—1st. The existence of a bronchitis of greater or less extent and intensity, even of the capillary variety, is not a contra-indication, and upon this all authors are in accord. 2nd. The existence of a pneumonia, either catarrhal or hypostatic, or of pulmonary collapse, or of hæmorrhagic infarction (Frölich, Jürgensen), is not a contra-indication; in these conditions it is indeed specially indicated. 3rd. The existence of more or less profuse diarrhœa. Jürgensen has noticed its diminution under this treatment. 4th. The appearance of the menses does not in any manner contra-indicate its employment. Currie, Brand, and all modern authors have never observed an untoward symptom under these circumstances. However, it is rare that the courses come on in typhoid fever. 5th. Epistaxis does not contra-indicate.

Finally, Lambert alludes to the aversion which some patients evince to the cold affusions. This is generally noticed in the first applications, particularly in regular practice. He recommends that the physician should firmly persist, not only because he is convinced of the necessity and urgency of the treatment, but also because all the patients, without exception, become readily accustomed to it, and are even impatient for the affusions. (*New York Medical Gazette*, vol. vi. No. 13.)

Treatment of Hepatic Cysts by Caustics.—M. Richet, in a recent lecture, states that the following plan is that which he now adopts after much experience and considerable reflection:—1. He makes an exploratory puncture for the purpose of

confirming his diagnosis; the liquid withdrawn being examined both chemically and with the microscope, and he still waits for spontaneous cure, which sometimes occurs. 2. In the event of this not happening, he proceeds to the application of caustics, and finds the Vienna paste the best, in the first instance, to open the way for that which he terms *le roi des caustiques*—chloride of zinc. By means of this last, he destroys the tissues of the abdominal parietes, layer by layer, till the peritoneum is reached; on arriving at this membrane, he punctures it with a small trocar, with a view of ascertaining the thickness of the parts that still remain to be perforated, and the amount of resistance they offer. A large trocar is now pushed through the centre of the eschar, and the canula is allowed to remain till the following morning; half the contents of the cyst only being allowed to escape at one time. The next day, or on the morning of the subsequent day, he withdraws the metallic catheter, and substitutes for it a supple gum elastic canula of the same size. By this means, he avoids the chance of any infiltration of the fluids contained in the cyst through the still soft adhesions that glue the cyst to the abdominal walls. During the few following days, if the opening appear to him to be insufficient for the ready discharge of the larger cysts, he increases its size by the introduction of a cone of prepared sponge, which opens a passage for a still larger canula—such a one, for example, as is termed a rectum canula on account of its being used to dilate contractions in the rectum. In addition to this operative procedure, M. Richet uses astringent injections, such, for instance, as a solution of tannin, mixed with a third or a fourth part of alcohol, with the hope of disinfecting the liquids of the cyst and of cleansing its walls. He considers it to be prudent to diminish the calibre of the canula very slowly, and does not give up their use altogether until satisfied that the walls of the cyst are sensibly modified and obviously approximated. (*Gaz. des Hôpitaux*, No. 47, 1872.)

The Treatment of Chorea.—M. Wenz, of Dorbach, states that a patient came under his care suffering from chorea of the right side. She was a girl of seventeen, who had not yet menstruated, and had been affected for several weeks. Several plans of treatment had been tried without effect. He determined to try what influence the local production of anæsthesia would produce. He accordingly directed an æther spray apparatus upon the spine, moving it slowly up and down till the skin was quite white. The girl passed into a cataleptic condition, with loss of consciousness. The quantity of æther used on the first occasion was about an ounce and a half, and the operation was repeated on several successive days, but with about half the quantity. The chorea entirely disappeared, and by further

remedial measures, directed to restraining the loss of blood, to which the disease was primarily attributable, a relapse was prevented. Wenz recommends a trial of this plan in tetanus; the spinal column being the part ætherised. (*Aerztliches Literaturblatt*, No. 7, 1871.)

Therapeutical Administration of Metals in Cod-liver Oil.—Herr Godin recommends, in place of the ordinary solutions of the metallic salts, their solution in cod-liver oil by means of benzoic acid. Cod-liver oil can thus be made to take up benzoate of iron and benzoate of mercury. Benzoate of iron is a beautiful orange-coloured salt of stable character, which increases the therapeutic activity of cod-liver oil. The iron salt conceals the unpleasant taste of the oil, and at the same time renders it more digestible. (*Therapeutisches Beiträge zur Allgem. Wiener Med. Zeitung*, No. 31.)

Therapeutic Value of Apiol. — MM. Joret and Homolle have obtained from the fruit of the common parsley, *Petroselinum sativum* or *Apium petroselinum*, a yellowish, oily, non-volatile fluid, to which they have given the name of apiol. This fluid is heavier than water, soluble in alcohol, æther, and chloroform, but insoluble in water. It has a peculiar and penetrating odour. In small doses it produces a powerful excitation of the system, a lively sensation of heat in the thorax and epigastrium, eructations, nausea, vomiting, and sometimes even colic; in doses of from thirty to sixty drops, it produces a kind of intoxication resembling that of haschisch or of quinine. It is recommended as a febrifuge in doses of fifteen grains or more; and though less powerful than quinine, it deserves to be employed. It has also been used as a remedial agent in amenorrhœa and dysmenorrhœa; its emmenagogue properties appear to be well established. The best mode of administering it is in the form of capsules. (*Cauvet, Histoire Naturelle Médicale*.)

Therapeutic Value of Carbolic Acid.—A long and remarkably well-written paper on carbolic acid, or carbol, as he prefers to term it, appears in the *American Journal of Medical Sciences*, by Dr. Bill, of the United States' army, and the following are the conclusions at which he has arrived:—1. It is not proven that carbolic acid is a general disinfectant. 2. It is of the greatest use to disinfect wounds. 3. It accomplishes this (a) by destroying pus, &c.; (b) by preventing inflammation. 4. Its use in wounds moderates pain. 5. Its use on the skin relieves itching, and produces an anæsthesia sufficient for minor cutting operations. 6. It seems to be of use internally, in certain cases, in scaly skin diseases; and at least as a moderator of pain in cancer. 7. It has not proved of decided use in other diseases.

From his experiments with carbolised catgut passed through the comb of a cock, he finds—1. That carbolised gut is more easily decomposed by contact with living structures than non-carbolised catgut. 2. The “living ligature” is merely the capsule which (at first thrown around the foreign body, possibly for its removal) has contracted into a firm cord.

Lastly, he states as the results of his researches upon the effects of the internal administration of carbolic acid in doses of from six to eight grains dissolved in a wineglassful of water, that there is: 1. A loss of sensibility in the mouth and throat, or a feeling of numbness, as when aconite is applied to the lips; this is followed by a cooling sensation like that of mint. 2. Slight nausea, especially if the stomach is empty. This is succeeded by an uneasy feeling in the abdomen, like that felt before an attack of gravel. 3. Slight vertigo, ringing in the ears, and partial deafness, judged by the ticking distance of a watch. This vertigo is so great if the carbolic acid be taken just after rising (and of course on an empty stomach) as to compel the resumption of the horizontal position. 4. Loss of heart-beat, the pulse from repeated observation on different individuals losing from four to eight beats in a minute, failing also in fulness. The temperature under the tongue undergoes no noteworthy alteration. 5. Diarrhoea. This is not invariable, and does not appear until several doses have been taken. If present, it usually disappears on the third or fourth day of the continued administration of the medicine. 6. After long-continued use of the medicine, feebleness of heart-beat, muscular debility, and loss of flesh occur. On omitting the medicine after it has been taken for several days, flatulence usually occurs, accompanied with a feeling of depression like that felt after the stimulating effects of morphia have ceased. (*American Journal of the Medical Sciences*, July 1872.)

Treatment of Parotitis.—Dr. Record observes that writers generally treat of mumps as a very trivial disease, hardly worth consideration, and that no doubt where it does not attack other organs by metastasis, it is very trivial. In the cases, however, which have happened to fall under his care during a recent epidemic, metastasis occurred in at least seventy-five per cent. In fifty per cent. it was transmitted to the testicles; in the others it went to the stomach, brain, &c. He did not witness its metastasis to the mammæ. He tried the treatment usually recommended, and obtained but little success. He then changed his plan, and if his advice were sought early, directed the patient to go about his usual avocations as if nothing was wrong, and *not* confine himself to the house, or tie up his jaws, or use any embrocations, liniments, or medicines whatever, as heretofore ordered, but just to consider himself safer with his mumps

uncovered and uncared for, than he would be while confined to a warm room, sweating himself with hot flannels, fomentations, or greasy compounds applied to his neck. In all cases where these directions were followed there was only simple parotitis, with no metastasis. In the later stages, when the treatment recommended in books has been already adopted, he proceeds to treat symptoms. If there is great pain in the head, a blister is indispensable. Where there is much fever he prescribes sedative diaphoretics, and uses fomentations to the scrotum. These, however, he regards as mere placebos, and places his chief reliance on turpentine. In every case, without a single exception, where this was administered, the disease was shortened, and his patients were happy and comfortable in from twelve to thirty-six hours. (*Cincinnati Lancet and Observer*, July 1872.)

Treatment of Typhoid Fever.—M. Péter gives every other day a glass of Seidlitz water, and every morning and evening an emollient injection, to remove putrid matters. He thinks this in no way exhausts the patient. To combat the fever, M. Péter orders seven or eight grains of quinine daily, augmenting the dose if requisite. He also gives vinous stimulants. Each patient has four ounces of quinine wine. The drink otherwise allowed is a pint or two of vinous lemonade. When the temperature is high, he does not object to the action of cold, but does not consider either cold baths or affusions advisable. He applies cold sponging with vinegar and water, the surface being rapidly dried after being moistened. (*Journal de Médecine*, October 1872.)

The Aural Diseases of Children.—Dr. Julius Böke, aural physician to the Hospital for the Diseases of Children at Pesth, observes that the bony labyrinth or the capsule for the terminal distribution of the auditory nerve is completely developed in the newborn child. Yet children hear nothing up to the third month, and it is not uncommon for the sense to be developed at a still later period. This is due to the condition of the membrane lining the tympanic cavity, which in the newborn child resembles a mucous membrane, whilst the cavity itself contains no air. Hence the membrana tympani of an infant is of a dull grey colour, and destitute of lustre; its position is also more horizontal, and the handle of the malleus is directed towards the tympanic cavity. The auditory meatus of the infant is still a membranous tube, and runs in a direction almost perpendicular to the median line of the body. This is the consequence of the mode of growth and development of the bones of the skull; the superior bony wall being formed by the horizontal part of the temporal bone, and the ossification of

the anterior inferior wall being first completed at the tenth year. These relations should be carefully borne in mind in cleaning and injecting the auditory passages of very young children.

Aural affections of children are most frequent between the ages of three and seven, and this is due to the circumstance that acute exanthematous affections, which are associated with affections of the tonsils and of the mucous membrane of the throat, are then most common. Such affections are apt to propagate themselves along the Eustachian tubes into the tympanic cavity, the lining membrane of which, as above stated, presents all the characters of a mucous membrane. Hence also the most common form of disease in childhood is otitis media. Primary disease of the external auditory meatus is more frequent before the age of seven than afterwards, when it is apt to accompany or be secondary to disease of the middle ear, rendering it difficult to determine which was first affected. Dr. Böke refers otitis externa to carious teeth, since the external auditory meatus and the jaw are both supplied by the same nerves and vessels. For the treatment of discharge from the ear he recommends injections of warm water and insertion of a plug of cotton-wool. Five drops of a lukewarm solution, containing two grains of acetate of lead in two ounces of a mixture of water and glycerine, may after each injection be dropped into the ear. The diseases of the middle ear are divided by the author into inflammation and catarrh. Inflammation in the tympanum is accompanied by very violent symptoms, fever and cerebral excitement, and leads to perforation of the membrana tympani and purulent discharge. If the discharge have only lasted a few days, it may be injected two or three times with lukewarm water; but if it have lasted longer, astringent solutions should be dropped into the ear. Polypi are to be touched with nitrate of silver which has been melted in a porcelain capsule and allowed to concrete to the size of a mustard-seed on the end of a probe. Treatment is required for the space of six weeks: perforations of the membrana tympani heal. Complete deaf-mutism developing in children is usually the consequence of brain-disease. (*Jahrbuch für Kinderkrankheiten*, Band i v. Heft 1: *Praktische Aerzt*, 1872, p. 165.)

Tannin-packing in Prolapsus Uteri.—Dr. G. P. Hachenberg, of Rochester, N. Y., states that for many years he has treated prolapsus uteri by packing tannin around the mouth and neck of the womb. He was led to adopt this mode of treatment in the management of the following case:—In 1858 a lady came under his treatment, having laboured under this form of uterine disease for fifteen years. She was married, and about fifty years of age. She had had an abortion, to which she

attributed her difficulty. Having ample means and intelligence, she consulted some of the most eminent obstetricians and surgeons of this country. She was not able to tolerate the pessary, or any other mechanical support, and no treatment, either local or constitutional, resorted to in her case for the prolapsus, gave her any permanent relief; the disease only becoming more aggravated, until the os lay low in the vagina, and at times was even exposed. When she came under Dr. Hachenberg's treatment, he found her health generally impaired, nervous, and subject to violent hysterical paroxysms of irritability of the stomach. There was no disease of the uterus, except a remarkable elongation of the cervix, with the os lying close to the external opening of the vagina. It was evident that the conical shape of the cervix, acting as a wedge on the walls of the vagina, caused the prolapsus.

After giving the patient the necessary constitutional treatment, and believing that as long as the cervix uteri was in its present shape and condition no relief could be given, he proposed to his patient the extirpation of the cervix by the aid of the *écraseur*. His proposition was submitted to Professor Sager, of the Michigan Medical University, who, after learning the history of the case, did not fully acquiesce in his view, leaving, however, the case to his judgment. The patient first consented to the operation, but afterwards objected to it, and requested him to try once more the effects of local treatment.

Of all local remedies to meet the indication of the case, he could think of none so favourably as of tannic acid. Taking its therapeutical properties into consideration as a powerful astringent—that it would corrugate without irritating the integuments—he conceived the idea of retaining it in its place by a thorough packing. This he accomplished in the following manner:—

A glass speculum was introduced into the vagina so as to push the uterus into its place. Through the speculum was introduced a metallic tube or syringe, with the end containing about thirty grains of tannin. With a suitable piston the tannin was now pushed out of the cylindrical tube against the uterus. The cylindrical syringe was then withdrawn, and the packing was neatly and effectually completed with a dry probang around the mouth and neck of the womb. After the packing was completed the probang was placed against the tannin in order to hold it, and the speculum was partially withdrawn. The packing was now fully secured. The probang was next withdrawn, closely followed by the speculum.

The application of tannin held the uterus firmly and securely in its place, not by dilatation of the walls of the vagina, as in case of the use of a pessary, but rather by an opposite condition—by corrugating and contracting the parts. The patient was

promptly relieved by the application, and to her great astonishment was able to take long walks with comparative comfort.

The happy effects of this packing continued about a week, when symptoms of a relapse began to show themselves. Another packing was resorted to, with the same good effects. As he proceeded with the treatment of the case he prolonged the intervals of the application. At first they were made weekly, finally but once or twice a month. In two years the cure was completed, and he understands that the lady enjoys comparatively good health since.

The almost constant application of tannin to the uterus not only overcomes the hypertrophy and elongation of the cervix, but he thinks even induces a slight atrophy of the parts. At no time did the patient suffer from this local treatment.

From its good effects in this case he was induced subsequently to continue the treatment by tannin-packing in other cases. (*Medical Record*, 1872, p. 342.)

A certain Sign of Death.—Dr. Hugo Magnus, assistant physician in the hospital at Breslau, has arrived at the following conclusions in his interesting essay published in *Virchow's Archiv* for Aug. 1872. The signs of death generally given by authors, such as incipient putrefaction, occur too late to be of any practical value. But indications of death drawn from the circulation of the blood and from the respiratory movements are of the utmost value, since if these have entirely stopped it is impossible that life can be maintained. Dr. Magnus recommends that a ligature should be tied tightly round some member of the body—a finger is well adapted for the purpose—when, if the slightest trace of life remain, increased redness will be observed in the part beyond the ligature. The tint gradually becomes darker and deeper till ultimately it assumes a bluish red, and this tint is uniform from the tip to the point where the ligature is applied, except that in the immediate proximity of the ligature is a white line. This test appears to be a good one, and is founded on physiological facts. (*Acztliches Literaturblatt*, No. 9, 1872.)

Notes and Queries.

DEPARTMENT OF ANALYSIS AND INVENTIONS.

THE NEW ASPIRATOR.

WE have pleasure in recording the trial and perfect success of the new modification of the aspirator just devised by Messrs. Weiss. This consists of the usual perforating needle (of various sizes), an india-rubber tube, with an interposed bit of glass tube to show at once the character of the extracted fluids, a glass vacuum bottle, a second piece of tubing, and a powerful brass exhausting syringe. The syringe exhausts the bottle; the needle is introduced; the stopcock towards the body is opened, and the fluid rushes into the exhausted bottle. When the latter is sufficiently filled, it is again cut off from the chest by turning one tap, and emptied by opening a lower tap and admitting air to the bottle by disconnecting the tube that joins it to the syringe. When the bottle is empty, the syringe is re-attached, and the bottle exhausted afresh; then the stopcock towards the body is again opened, and another bottleful of fluid withdrawn: and so on for as many times as may seem advisable.

On Oct. 24th this instrument was applied to a patient of Dr. Basham's, in Westminster Hospital. The fluid had collected very insidiously. Although the left chest was completely full, with great bulging of the ribs and displacement of the viscera, the symptoms were comparatively quite trifling, and the man could walk about freely with but small distress. The perforating needle was introduced by Mr. Pearse at the lateral aspect of the chest, the man lying on his back. A somewhat clouded serous fluid, with flakes of plastic matter, issued forth, and one hundred ounces were extracted with great facility in the course of about half-an-hour. It is impossible to wish for anything more satisfactory than the whole proceeding. Beyond the inevitable slight cough at first, and a sense of faintness (at once relieved by brandy), the patient lay as calm and undistressed as if nothing were being done to him; and we understand there has

been only very trifling disturbance since the operation. It was necessary once, during the operation, to change the perforating needle for a larger one; but this was effected in a few seconds with the utmost ease.

CORRESPONDENCE.

ON THE TREATMENT OF STRUMOUS OPTHALMIA BY BELLADONNA GIVEN INTERNALLY.—Dr. James Braithwaite, Leeds, writes to us as follows:—"The publication of the interesting paper by Mr. Power in the October number of this journal, in which the administration of belladonna internally in cases of strumous ophthalmia is recommended, has induced me to communicate the results of my experience on this point.

"For many years I have been in the habit of treating cases of strumous ophthalmia with belladonna internally, and with such almost uniform success that I am rather glad to get a fresh case instead of dreading the disease as I believe many people do. I have no special opportunities of observing the disease further than those given by private practice, and must give the credit of the successful treatment to my father, who first pointed it out to me.

"My practice has, however, differed from that of Mr. Power in several important particulars. I believe—1. The belladonna should be given at once without waiting till other means fail. 2. Its efficacy is much increased by combination with iodide of potassium. 3. Atropine should not be applied too early, for sometimes it produces considerable irritation and increases the severity of the photophobia instead of relieving it. Instead of this, extract of belladonna rubbed up with a little glycerine should be spread over the eyelids, eyebrows, and temples. This plan is not so cleanly and gives more trouble; but it is certain to do no harm, and produces the same effect on the pupil as atropine, but more slowly. I can only remember one case in which no good was done by this plan of treatment, and it was only under my care ten days, when, becoming dissatisfied, the parents took the child to a quack who promised more immediate results. I generally give the extract in an eight-ounce mixture containing four grains, and half a drachm of iodide; and of this, for a child two years old, the dose should be two teaspoonfuls every four hours. If no improvement result in a few days, the dose may be increased somewhat, and the next time the mixture is repeated the belladonna may be increased to five grains, and the iodide to a drachm. I believe cod-liver oil to be very useful, but have thought iron injurious.

Change of air to the country, and counter-irritation with iodine paint, are often required in order to get rid of the last symptoms of the affection."

ON THE PROPER SIZE OF THE TROCARS FOR THE "ASPIRATOR."
—Dr. Clifford Allbutt asks us to publish the following correspondence:—

"BOSTON, U.S.A., Sept. 12, 1872.

"DEAR SIR,—I always read any paper to which I see Dr. Clifford Allbutt's name prefixed. Accordingly, I read with avidity what you have to say on thoracentesis,¹ and I heartily agree with you about the propriety of *early* tapping. I think I have sometimes delayed a little too much; I shall do so no longer.

"But I write chiefly with reference to one phrase in your paper which I cannot really comprehend. You say, p. 79, 'The ordinary-sized Bowditch canula is rather *too large*,' &c. Pray how large are English 'exploring' trocars? For an American 'exploring' trocar, simply made thick enough to safely thrust between the ribs, is what I have used ever since I first operated in this way, as suggested by my friend Dr. Wyman.

"An 'exploring trocar' and 'suction pump' are essential points of Dr. Wyman's idea. I hope thoracentesis (*i.e.* when used freely) will be always performed with such a trocar, unless in some cases where pus already exists. In these I believe we must generally make *long* incisions between the ribs, and not trust to large trocars or drainage tubes, &c.

"I remain, very respectfully,

"HENRY J. BOWDITCH."

[I have sincerely to apologise to Dr. Bowditch for my inaccurate use of his name. Of Dr. Bowditch's own trocars I knew nothing, and when an operator attains the familiar place and distinction of Dr. Bowditch, his name is applied to many a thing not his own. In England, almost any exhausting trocar and canula with an apparatus to prevent the ingress of air gets rightly or wrongly the name of a "Bowditch trocar,"—often no doubt wrongly. These vary very much in size, and are often, no doubt, made as small as those used by Dr. Bowditch himself. But they are often much larger, and, I think, with the hope of preventing occlusion. My point is that, paradoxical as it seems, the larger tubes are the very tubes which do become occluded by fibrinous coagulation.

I cannot be sorry, however, to have drawn forth so valuable an expression of opinion from perhaps the first living authority on the subject.

T. C. A.]

¹ *Practitioner* for August.

THE PRACTITIONER.

DECEMBER, 1872.

Original Communications.

ON THE OPERATIVE TREATMENT OF ECTROPIUM, WITH ILLUSTRATIONS.

BY W. SPENCER WATSON, F.R.C.S.,

Surgeon to the Royal South London and Central London Ophthalmic Hospitals.

THE treatment of the inflammatory or acute form of ectropium, and that of the senile and paralytic forms, is so fully discussed in the text-books of Ophthalmology that little or nothing new can be written on this subject. In cases of cicatricial ectropium, however, depending upon loss of substance of the skin of the eyelids or the neighbouring parts, the method of operating requires modification in almost every particular instance; and as the remarks that follow are deducible from my own experience in certain cases of difficulty, I trust that I may thus add something to the stock of existing materials and give precision to the rules for operating.

Of the cicatricial form of ectropium *three classes* of cases are met with requiring operative interference.

CLASS I.—Those in which the eyelid is everted by the contraction of a cicatrix at some distance from the skin of the eyelids, the eyelids themselves being sound and unaffected, or at most only slightly elongated. For example, a scrofulous abscess involving the malar bone has healed and left a depressed cicatrix; and in the course of contracting has drawn the lower lid downwards

and outwards, exposing the mucous membrane; or an abscess in the supra-orbital region leaves a puckered cicatrix and draws the eyelid towards it. This condition represents the simplest forms of ectropium with which we have to deal surgically, and the operation required is a subcutaneous dissection of the depressed cicatrix, liberating the skin around it freely by further subcutaneous dissection, and then bringing the deep surfaces of the reversed skin into apposition by twisted suture, or two crossed hare-lip pins.

The eyelid will then probably be reducible, but if the distortion has been of long standing the cartilage is elongated and widened in every direction, and it will then be necessary to excise a V-shaped portion of the whole thickness of the eyelid, the open end of the V being at the tarsal margin. The edges of this gap are brought together by twisted suture, and the whole covered by cotton-wool and collodion or Richardson's styptic colloid.

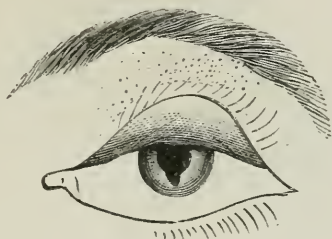
A case of ectropium of the lower eyelid treated in this way by Mr. Hulke at the Central London Ophthalmic Hospital answered extremely well, and in the following case of ectropium of the upper eyelid, under my care at the same hospital, a somewhat similar method was adopted.

Case.—A Frenchwoman, thirty years of age, had eversion of the left upper eyelid from the contraction of a cicatrix close under the centre of the eyebrow. This, she stated, occurred after an attack of erysipelas with subsequent abscess some years before. The upper eyelid was completely everted and the tarsal cartilage very much elongated, especially at its free margin, and proportionally widened in a vertical direction, but very little loss of the skin-substance had resulted. The following operation was performed while the patient was under the influence of bichloride of methylene.

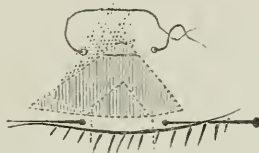
First Step: A semi-lunar incision was made parallel with the tarsal margin 8''' in length, and on the cutaneous aspect of the eyelashes; the skin was then dissected back from the cartilage upwards towards the cicatrix, which was also freed in the same dissection, and the central portion of the skin and muscular structures of the eyelid liberated in a lateral direction by subcutaneous dissection. An oval gaping raw surface was thus

exposed, with the tarsal cartilage occupying its base but allowing the latter to be turned down towards the eyeball (see Fig. I).

I



II



Second Step: A V-shaped portion of the cartilage thus denuded was then excised, and the edges of the gap with the skin near the margin of the lid brought together by means of a twisted suture over an entomological pin; another suture, passed across the upper part of the gap in the skin horizontally, converted the first longitudinal incision into a vertical line of union; the centre of the upper line forming the apex of the parts united. The whole of the upper eyelid was now covered with cotton-wool saturated with styptic colloid (Fig. II.). The sutures and crust remained attached for a fortnight, and at the end of that time perfect union had resulted.

Two months afterwards the eyelid was in very good position, with scarcely any trace of the operation visible except on close inspection.

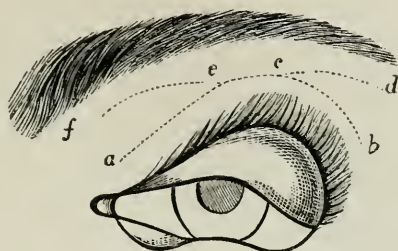
In another case of partial eversion of the upper lid by a cicatricial band attaching it to the outer and upper margin of the orbit, a triangular flap of the skin and muscle (with its base towards the everted edge and the apex towards the cicatrix) was first made and dissected downwards; then the edges of the gap

left were brought together by sutures. The result was only a partial improvement of the condition of the patient, and not by any means so good as in the case last related. In a similar case I should prefer the method adopted in that of the French-woman, even when the eversion is only partial.

It may in some such cases be desirable, on account of the greater eversion of one portion of the eyelids, to remove the V-shaped portion from the part most affected (Adams' operation); and if it should happen that the adjacent parts of both upper and lower lid are dragged away from the eyeball and everted, it will be preferable to remove the ciliary margin and a portion of the conjunctiva of both, uniting the raw edges subsequently by sutures. This constitutes the operation of tarsoraphia (of Walther), and may be employed in any of the less complicated forms of ectropium, and in the worst cases is sometimes a useful addition to the principal steps. Dieffenbach's method of shortening the lid (see Zeis' "*Handbuch der Plastischen Chirurgie*," p. 378) is preferable in those cases in which it is not desirable to diminish the size of the palpebral aperture, or in which the cicatrix is situated at the outer canthus, and could be included in and removed by the incisions.

CLASS II.—The next class includes all cases in which, besides the eversion, there is loss of substance of the skin and other tissues of the lid. Burns, ulcers, scrofulous ulcers, with or without caries of the orbital bones, and cicatrices after injuries or operations, are each of them capable of producing this form of ectropium. Here the operations required are much more complicated, and almost every case will call for a special plan. In the majority, however, the rule would be to utilise the nearest available healthy skin in the form of flaps. The simplest case is that of a puckered cicatrix of one portion of the upper lid, by which the tarsal margin is everted and drawn up to within a short distance of the superciliary ridge, the intervening skin having been destroyed by previous destructive ulceration, but the remainder of the skin on each side being sound and of nearly normal dimensions vertically. Under these circumstances triangular flaps taken (as represented in the diagram III. *a*) from the sound tissues of the lid may be made to fit mutually into the receding angles formed in the course of

the dissection and retained in position by sutures, as shown in Fig. V., which represents the upper lid with the lines of union after the flaps had united. It is necessary, however, first to



III a

In the above figure the operation on the upper lid is represented. The first incision being carried along the line *a, e, c, b*, parallel with the tarsal margin, the two flaps *a, e, f*, and *b, c, d* were then liberated, and the tarsal cartilage also liberated by a free dissection downwards and backwards. The skin above the line *f, e, c, d* is also freely dissected upwards. A gap is thus left into which the two lateral flaps are brought, the point *c* of the flap *b, c, d* being attached in the retreating angle at *a*, and the point *c* brought over and united by a suture in the angle at *d*, or as near it as possible. The lid was now in a good position, and was kept so by the increased vertical depth of skin between the tarsal cartilage and the eyebrow, as seen in Fig. V. It was necessary in this particular case subsequently to remove a piece of hypertrophied conjunctiva which protruded below near the outer canthus, after which the condition of the eye was that represented in Fig. V.

dissect in a line parallel with the ciliary margin of the tarsus, and to disengage the tarsus completely from the skin and other tissues, and bring it down with the eyelashes attached to its normal position covering the eyeball.

The raw surface left is then to be filled up by the triangular flaps in the manner above described; and if any portion of the cicatrix is adherent to bone, this should be freed, and in some cases the dense cicatrix tissue had better be taken away altogether; taking care, however, to sacrifice as little skin as possible. Should there be any dragging on the flaps after the sutures have been put in, it will greatly facilitate union by the first intention to pass a long subcutaneous suture beneath the flaps, from the temporal side to the supra-orbital region, each

end of the suture being secured over a roll of sticking-plaster or a piece of bougie. The whole of the flaps should then be covered with collodion, which by its contraction also prevents dragging on the sutures, or by cotton-wool saturated with Dr. Richardson's styptic colloid. These dressings have a threefold advantage: (1) they prevent movement of the lid by keeping it in a firm case, (2) they keep the edges of the flaps accurately in apposition, and (3) they do not require to be changed, unless indeed suppuration should occur, and even then it may be necessary only to remove a portion in order to allow the escape of discharges, while that portion which remains, still holds the flaps together and favours union in the best position.

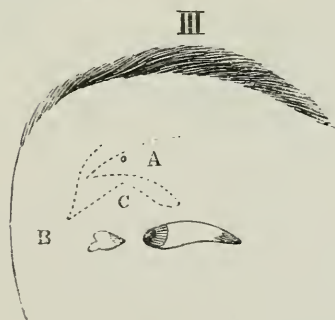
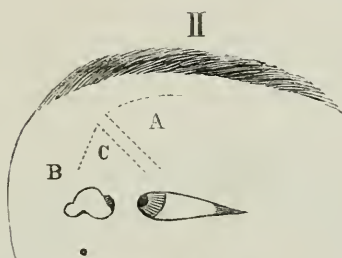
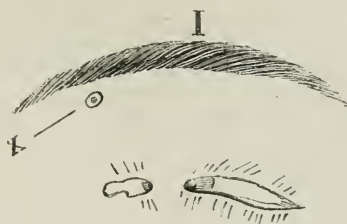


V

Whenever the ectropium has been of long standing, the conjunctival sub-mucous tissues have become thickened, and so prevent the lid from coming down into position even after a free dissection such as described above; and it is then necessary to remove a portion of the thickened membrane, the size of the piece removed being proportionate to the amount of displacement it is producing. Should the lid be elongated from long-continued stretching, it may become necessary to excise a V-shaped portion of the cartilage as described for Class I.; but

this is seldom required for eversion of the upper lid, though not unfrequently for the lower, the tissues of which, being more distensible, suffer more from the constant dragging of a cicatrix.

The operation above described by the use of two triangular flaps is one which is not described in any of the text-books on



Ophthalmology, though the operation ascribed to Dieffenbach is the nearest approach to it. They have been in use for some years by the surgeons at the Royal London Ophthalmic and by myself at the Royal South London Ophthalmic Hospital the diagrams annexed being taken from a case under my treatment

at the latter hospital. I believe the principle of using sliding flaps of integuments is of German origin, and has been adopted in this country more especially by Messrs. Bowman, Critchett, and Hulke. I have witnessed operations by all these gentlemen in which the triangular flap was used as I have described.

In dealing with the less severe forms of eversion it may sometimes be possible to employ only a single flap; as, for instance, when the eversion is slight and the cicatrix limited in extent. The vertical depth of skin being little less than that of the normal eyelid, a single flap properly placed with a sufficiently broad base will answer well, as it did in the case (one of Mr. Bowman's) to illustrate which the diagrams annexed were made (see preceding page).

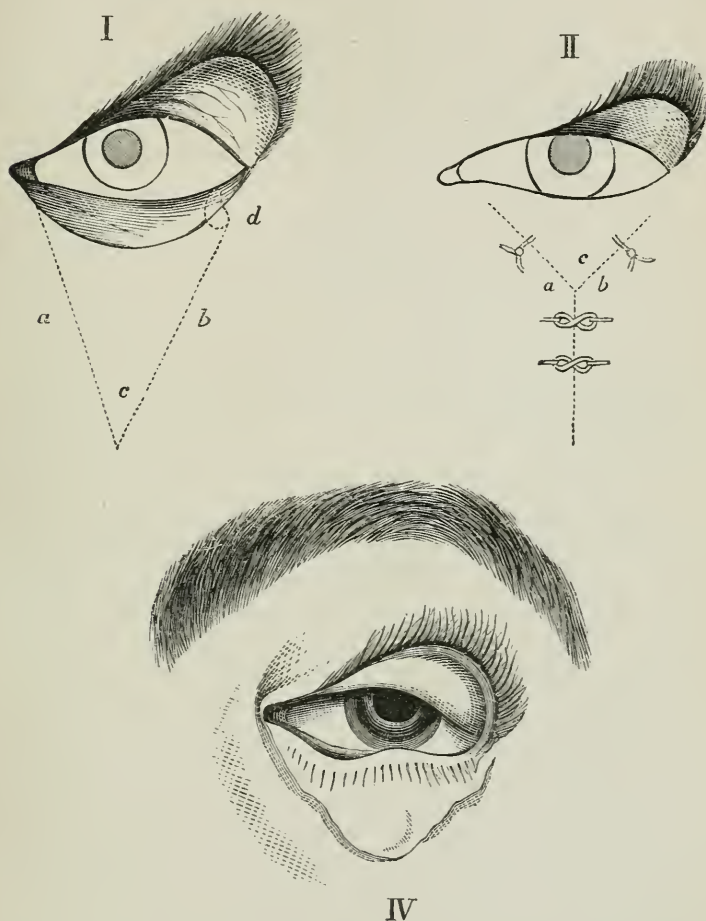
In this case a further modification was employed, viz. that of previously uniting the edge of the upper and lower lid by paring a small portion of their edges just internal to the ciliary margin, and retaining them in position till union had taken place. This ingenious proceeding much facilitated the subsequent operation, and of course, when the flaps had become quite united, it was very easy to detach the point of union of the lids.

In dealing with the lower eyelid a larger choice of operations is possible, and it is sometimes impossible to find healthy skin available for two flaps close to the eyelid. Under such circumstances the skin of the cheek may be utilised, as in the operation known as Mr. Wharton Jones's (see Figs. I., II., and IV. on the next page). In a case from which I append drawings this plan was adopted, together with excision of a piece of the mucous membrane and cartilage (see Fig. I. *d*) of the everted eyelid, with a fair amount of success.

In reference to this case it is worthy of remark that a portion of the long flap from the cheek sloughed at the pointed apex of the triangle. In such another case, with a flap of such length, I should prefer giving the apex a rounded form, as I believe there would then be less risk of sloughing. The sharper the point of a flap, the greater must be the risk of its suffering from defective nutrition, not only from the difficulty in maintaining its temperature till union has taken place, but from the greater possibility of its nutrient vessels becoming isolated from their main trunks. When the skin of the lower eyelid is only

partially spoilt by cicatrix, it is possible to restore its position by the two sliding flaps in the following way:—

First Step: The cicatrised margin and its adjacent mucous membrane are dissected off in a triangular piece with or without the attached cartilage. Second Step: A triangular flap of skin is



made by an oblique incision commencing at the termination of the raw surface at the edge of the tarsus downwards and inwards (or outwards if the skin is more healthy in that direction), and the incision then carried to the required length beyond the end of the flap, in a line with its upper boundary. The second flap

is formed of the tarsus above this last incision, and this, being freed by dissection from its attachments, is brought into apposition with the raw surface made in the first step, and attached there by sutures, the lower flap being also retained by sutures in the retreating angle beneath the second flap, or that formed of the skin and other tissues of the eyelid itself.

By this means no sound skin is sacrificed, and a shortening of the lid is effected by the removal of that portion only which has been spoilt by cicatrix. Such an operation can only be admissible when the destruction of the tarsal margin is somewhat



limited in amount; as, for instance, when one-fifth or at most one-fourth of the inner or outer extremity of the margin has been destroyed.

(To be continued.)

NITRATE OF AMYL IN ANGINA PECTORIS.

BY W. HERRIES MADDEN, M.D., F.R.C.P.,
TORQUAY.

HAVING, quite recently, experienced in my own person the remarkably beneficial action of the nitrate of amyl in cutting short attacks of angina pectoris, I think it may be interesting to the readers of the *Practitioner* to be told what I felt. Nor will the record, I hope, be without some little therapeutic value. On one point I individually have certainly gained an increase of knowledge. From the accounts I had read of the action of the drug, I had formed the opinion that it was only suited to those cases in which the face was pallid during the paroxysm, and as mine was flushed I dismissed from my mind all thoughts of trying it, and paid the penalty of hasty conclusions in the shape of a large amount of acute suffering.

A few words regarding my medical history may be desirable. My age is fifty-seven. My father died of angina pectoris in 1839—the organic cause in his case, as ascertained *post mortem*, being atheromatous obstruction of the coronary arteries. In the autumn of the same year, being at the time engaged in a large country practice near Edinburgh, and very hard worked, I broke down in health, with obscure heart-symptoms and threatened lung mischief. By the advice of my friends I left Scotland and came to Torquay, and after two years was so far recovered as to be able to practise. Since then I have led a busy, active life, often suffering much, especially from intense headaches, but very rarely laid aside.

In the winter of 1859 I had some recurrence of my old heart-symptoms, and, being in town in the next spring, took the opportunity of asking my friend Dr. Williams to overhaul me.

He very kindly did so, and found slight mitral incompetence. The knowledge of this fact made me more cautious than I had hitherto been. I avoided, as far as possible, all undue bodily exertion, and was able to get through my work comfortably. In the spring of 1871 I had an attack of bronchitis, with great nervous prostration; but by autumn I was well again, and accomplished the labours of the winter and spring without difficulty, though I had a good deal of professional anxiety and much painful worry of a different nature.

On July 8th, feeling perfectly well, I was walking slowly *down hill* to keep an appointment for which I was rather too early, when suddenly, without the slightest warning, I was arrested by a severe attack of angina pectoris, the pain extending across the front of the chest, along the inside of the left arm, and across the chin. In the evening a medical friend was sitting with me, and I asked him to examine my heart. He found a loud rasping systolic murmur, heard most plainly at the apex and at the inferior angle of the left scapula. He very wisely counselled rest, and I as foolishly did not take his advice, but persevered with my ordinary work, until the frequent recurrence and increased severity of the attacks compelled me, in about ten days, to give in. At first it seemed as if the quiet would prove curative, but in the afternoon of the fourth day after taking to bed I woke out of a doze with the severest and most prolonged spasm I had yet experienced. From this time the disease appeared to acquire increased violence. The attacks lasted, for the most part, for a quarter of an hour or twenty minutes, and recurred frequently at intervals of about three hours. Various remedies were tried, but with little or no benefit. Hypodermic morphia was the most useful, but it was impossible to employ it often enough without producing dangerous narcosis.

At this time, when I was getting thoroughly worn out by the constantly recurring pain, a friend, who happened to have in his possession a specimen of the nitrate of amyl, suggested to one of my kind attendants the desirableness of giving it a trial, and furnished him with a small quantity. He consulted with his colleagues, and they unanimously advised me to make the experiment. I was willing enough to do so, and the opportunity was not long wanting. That night, as usual, I was roused out of

my first sleep by a sharp attack. I at once inhaled five drops, and the effect was truly wonderful. The spasm was, as it were, strangled at its birth. It certainly did not last *two* minutes, instead of the old weary *twenty*. And so it continued. The frequency of the paroxysms was not diminished for some time; but then they were mere bagatelles as compared with their predecessors, and consequently the drain upon the vital energies was greatly reduced. Under these improved circumstances, strength gradually returned; the attacks became less and less frequent, and finally ceased. At the time of writing these lines (October 11) I have not had an attack for five weeks, and have resumed my ordinary duties, of course with care.

I cannot profess to give a full scientific description of the phenomena presented by the nitrate of amyl in action. The presence of intense pain is not favourable to the exercise of calm, philosophic analysis, and I can only tell what I *felt*. The first effect was often bronchial irritation causing cough; then quickened circulation; then a sense of great fulness in the temples, and burning of the ears; then a violent commotion in the chest, tumultuous action of the heart, and quick respiration. The angina pain then died out first in the chest, next in the left upper arm, and last of all in the wrist, where it was usually extremely severe. In speaking of my first experiment with the amyl, I said the spasm was as it were *strangled*; this word accurately expresses the sensation. I felt as if a new power was suddenly called into play, which seized hold of and by a violent effort crushed out the force previously in action. It was not by any means, in itself, a pleasant process; but I delighted in it, for I knew the end would be relief. When the pain had ceased there was generally for some time a strong involuntary tendency to suspension of breathing, each prolonged pause being followed by a very deep inspiration. There was not at any time the slightest confusion of thought, or disturbance of vision, but occasionally slight and transient headache.

As regards physical signs, the rasping sound was soon modified; but a loud blowing systolic murmur, heard at the base of the heart, along the aorta and in the subclavians, especially the right, continued throughout the illness.

I have omitted to mention one curious feeling which I

commonly had. The front of the chest seemed to be bulged out in a convex prominence, which suddenly terminated at the lower end of the sternum in a sharp and deep depression towards the spine. This was a purely subjective phenomenon. There was no contraction of the diaphragm, and no retraction of the abdominal walls. But though the hand laid upon the parts convinced my mind of their normal condition, it in no way modified the sensation.

All these things appear to me to indicate the nervous system as the chief field of action of the amyl. In *slight* commencing attacks, merely smelling the cotton-wool on which a previous dose had been poured was sufficient to relieve the pain. It acted like a gentle anæsthetic without any quickening of the circulation. But in a *severe* attack, the full action of the drug, with its concomitant vascular commotion, was quite essential. The pain never began to yield until the heart was violently affected. I soon learned to know when I had taken enough, and probably thus avoided unpleasant after consequences.

ON THE TREATMENT OF ACUTE MANIA BY THE SUBCUTANEOUS INJECTION OF THE COMBINED ACETATES OF CONIA AND MORPHIA.

BY J. WILKIE BURMAN, M.D. EDIN.,

Deputy Medical Director, West Riding Lunatic Asylum, Wakefield.

I HAVE often thought that the great therapeutic desideratum, in cases of acute maniacal excitement, is a drug that, whilst acting upon the motor and ideational centres in the brain in a sedative manner, will do so with the least possible disturbance of the functions of the *primæ viæ*, the proper performance of which is, in such cases, of so great an importance; and, of all the drugs at present used in cases of mania, chloral hydrate seems to be the one which possesses those qualities in the most eminent degree, and, as such, it certainly, I think, well merits that more extended use to which, no doubt, it will soon attain; but as I have, elsewhere,¹ gone into that subject, I need not here expatiate upon it. I am desirous, rather, at the present time, of drawing attention to another drug, viz. conia, which, when administered subcutaneously, either alone or in combination with morphia, I have found very serviceable in cases of acute mania, and which, when so given, has its own peculiar advantages.

The succus conii, administered internally, has been shown by Dr. Crichton Browne, of the West Riding Asylum, to give excellent results in cases of acute mania;² but further experience of it has shown that it is a preparation whose strength, for various reasons, varies considerably from time to time, as new

¹ *Lanæct*, vol. i. 1872, p. 356.

² *Ibid.*, February 3, 10, and 17, 1872.

lots of it are obtained : and this, with other motives, led to my looking for, in conia—the active principle of the drug—a stronger and more stable preparation of it. And though my experience of conia has shown that the alkaloid itself, as it is at present prepared, varies considerably in strength, yet I believe that a further demand for it would make it worth the while of chemists to pay more attention to the matter, and would lead, eventually, to the production of what cannot, at the present time, be procured, viz. a stable and crystallisable salt of the alkaloid. This variableness in conia itself is, to a great extent, attributable to the fact that it can only, as yet, be obtained in a liquid condition, and it is thus liable to be diluted or impure ; nor could we, indeed, well expect to get it pure and stable until it can be presented to us in a crystalline form. In the meantime, such as it is, conia seems to me to possess the following advantages over the succus conii, viz. : (1) that, being very much stronger and less bulky, inconveniences due to a variableness in its strength can be more readily provided against by regulating the dose administered ; and (2) that it can be administered subcutaneously, and thus, whilst leading to more speedy remedial effect—a very desirable result in most cases of acute mania—be more serviceable in such cases as those in which the patient will either not take medicine by the mouth or cannot retain it in the stomach. On the other hand, conia is, so far as it is yet known, unsuited for administration by the mouth, and, in those cases of mania where smaller doses suffice and where the patient will take all food and medicine, it cannot be denied that a *good* juice of conium, in convenient doses of from fl. ʒij to fl. ʒij, administered by the mouth from twice or thrice a day to every four hours, leads to capital results ; and indeed, under such circumstances, it would be manifestly unwise to resort to subcutaneous injection at all. In these cases, also, and with a good succus, very much the same results as those got from the subcutaneous injection of the combined acetates of conia and morphia can be obtained by the combined use of the succus conii and tinctura opii given internally.

In considering the action of conia and morphia when subcutaneously injected, in combination, I must necessarily first briefly refer to the effects of conia injected alone ; and by

"alone" I mean not in combination with another sedative, for acetic acid (or hydrochloric) must be added in order to neutralise the alkaloid (which is of a very strong alkaline reaction), and spirit and water must be further added to make the solution of convenient strength. The following is the formula I have adopted :—

R. Coniæ fl. ℥iij, ℥xij.

Acid. acetic. fort. fl. ℥iij, ℥xl, or a sufficiency to cause neutrality.

Spirit. vini rect. fl. ℥j.

Aquæ destillatæ ad fl. ℥ij.

℥v of the solution = ℥j of conia.

The acid must be added cautiously, the mixture being tested from time to time with litmus paper to determine its reaction.¹

As I have elsewhere shown,² conia may, in cases of acute mania, be subcutaneously injected, prepared according to the formula above given, in doses commencing with $\frac{1}{3}$ th of a minim, and varying from that to ℥iij of conia—or from ℥j to ℥xv of the solution—according to the amount of motor excitement and strength of the conia used. The invariable physiological effects of conia thus administered are—weakness and partial paralysis of the legs and arms, which gradually intensify and reach a climax in causing a general feeling of languor and lassitude, which leads to, nay, almost enforces, quietude; and, though only indirectly affecting the mental faculties, it predisposes to sleep without either causing any great derangement of the cerebral circulation or interfering in any way with the digestive functions. These effects I have repeatedly experienced in my own person after subcutaneous injection of doses of from $\frac{1}{5}$ th to ℥ij of the alkaloid. With regard to the succus conii given internally, Dr. John Harley³ has, by a beautiful process of inductive reasoning, shown that these effects are due principally to a direct action of the drug on the corpora striata—the motor centres within the cranium. The therapeutic effect of the drug, so used,

¹ For further details as to conia and the best methods of preparing and preserving a solution of it for subcutaneous injection, I must, at present, refer the reader to my paper "On Conia and its Use in Subcutaneous Injection" in vol. ii. of the West Riding Lunatic Asylum Medical Reports. Churchill, 1872.

² See last footnote.

³ "The Old Vegetable Neurotics," p. 11. Macmillan and Co., 1869.

is a rapid subdual of motor excitement, which wards off emaciation and exhaustion and promotes recovery, as shown by the results in nine cases of mania treated by me in this manner, and recorded in the paper already referred to.

Though, as I have said, *mental* quiet and sleep are often indirectly produced by the soothing of muscular irritability and feeling of general lassitude induced by the drug, yet it has no direct soporific effect or action on the mental faculties; and, *quoad* the *mental* excitement in cases of acute mania, treatment by it alone is, in fact, purely expectant. Looking at the matter broadly, we may say that the tendency in cases of acute mania is either to exhaustion due to excessive motor excitement (which is, in its turn, the outcome of the exaggerated and abnormal mental activity), or to a recovery more or less permanent and limited; and I attribute the good results which I have experienced by treating cases with conia in this way, solely to the lessening and controlling of the *motor* excitement, which leads to the preservation of the bodily strength of the patient, whilst the *vis medicatrix nature* does the rest, assisted by careful nursing and (his digestive functions being in no way interfered with) a liberal diet.

Conia, then, acting on the motor centres within the brain, and morphia on the ideational centres and sensorium, in a sedative manner, it has occurred to me that the use of the two drugs in combination would lead to results directly antagonistic to the condition of maniacal excitement. The one would seem to supplement the other, in such cases, in bringing about a complement of sedative action—conia acting directly on the motor and indirectly on the ideational centres, morphia acting directly on the ideational and indirectly on the motor centres; or, as Dr. Harley puts it,¹ “just as opium tranquillises and refreshes the over-excited and weary brain, so does conium soothe and strengthen the unduly excited and exhausted centres of motor activity.” Moreover, as I have ascertained by experiment and shall shortly show, the action of each drug is intensified by the combined use of both in subcutaneous injection—which quite agrees with the results Dr. Harley got by the use of the *succus conii* and *tinctura opii* in combination; so that, both on

¹ Loc. cit. p. 12.

account of this mutual intensification as well as the intrinsic assistance of the conia, it is not requisite, in treating cases of mania in this way, to give the morphia in such doses as to cause the headache, vomiting, or interference with the digestive functions which so often result from the larger doses of it that require to be given when morphia alone is injected; and this is an additional advantage which the combination presents.

In order to illustrate the prolonged and the additional effects, when conia and morphia are subcutaneously injected in combination, as compared with the effects when conia alone is given, I may perhaps refer to the results obtained in the cases of two rabbits, in which I was fortunate enough to have the maximum of effect without fatal result, and both of which were about the same weight—one weighing 2 lb. 13 oz., the other 2 lb. 14 oz.—and in the same condition. Rabbit No. 1 had \mathcal{M}_j of conia injected in \mathcal{M}_v of a solution according to the formula given; in seven minutes the hind legs were observed to be affected, and it jumped about with difficulty; gradually the paralysis extended upwards and throughout the body until, in eighteen minutes, the rabbit lay on its side, in a state of complete muscular prostration, without any depression of the heart's action, which was rapid and strong, and without any apparent loss of consciousness; it remained in this condition up till one hour and eighteen minutes after the injection, when it commenced to improve, and three-quarters of an hour later (or two hours after the injection) it was quite recovered and as lively and well as usual; there were, during this period, occasional general convulsive twitchings of the muscles—death from conia, in rabbits, being always preceded by convulsions. Rabbit No. 2 had \mathcal{M}_v of a solution injected = \mathcal{M}_{ss} of another conia (which I proved to be just twice as strong as that given to Rabbit No. 1) combined with gr. $\frac{1}{8}$ of acetate of morphia: the effects came on in much the same manner as in Rabbit No. 1, but there was a superadded condition of drowsiness and a decided prolongation of the effects; for though Rabbit No. 1 was quite well two hours after injection, this one lay on the floor for three hours in a drowsy and semi-paralysed condition, and it was not until four hours after injection that it had quite recovered. These results of the combination would, however, seem only to occur where

the dose is not of a fatal strength; for when such is the case—as in that of a rabbit weighing 5 lb. 2 oz., to which I administered, subcutaneously, ℥iiss of conia and gr. $\frac{3}{8}$ of acetate of morphia in ℥xv of solution, and which died in twenty-three minutes with the usual conia symptoms alone—the action of the conia comes on before that of the morphia and outruns it to the end; whilst when the dose is not fatal, and the combination proportionately the same as above stated, the morphia seems to prolong the effects by taking up its action about the time that the conia commences to fail; and it is thus that morphia supplements the action of conia. These effects of the combination I have also verified by subcutaneous injection in the healthy human adult; for in the case of a colleague of mine, who had before had eleven injections of a solution of conia uncombined, and who, consequently, knew well the effects of conia alone, it was observed by himself, after the injection of ℥v of a solution = ℥ss acetate of conia and gr. $\frac{1}{8}$ of morphia, that the effects were much more prolonged than after a similar dose of conia uncombined with morphia.

The following formula for the preparation of a combined solution of the acetate of conia and morphia for hypodermic use is that which I have always adopted, on account of its convenience, both with regard to the proportion of the ingredients, as well as the absolute bulk of the solution required to be injected to get the desired effect. Take of a solution of conia, prepared as before, ℥j in ℥v, and of a solution of acetate of morphia (gr. $\frac{1}{4}$ in ℥v), equal parts, and mix: the result is a clear solution, ℥v of which are equivalent to ℥ss of conia and gr. $\frac{1}{8}$ of acetate of morphia. A solution of acetate of morphia of this strength is generally turbid at ordinary temperatures, but, on being added to the solution of conia, it quickly clears up and remains clear.

A combined solution of conia and morphia, prepared with the best conia and as above directed, may be subcutaneously injected in cases of mania, in doses of from ℥iiss to ℥x or more, cautiously increasing the dose if necessary. With the conia I am at present using, and the proportional strength of the ingredients being as above stated, I have not as yet injected larger doses than ℥x of the combined solution; but so far as one may judge from the fact that a single dose of ℥xl of it = ℥iv of

conia and gr. j of acetate of morphia, injected in two parts under the skin of the back, did not prove fatal to a large retriever cur weighing about four stone, though, of course, there was well-marked muscular prostration, with drowsiness and running of saliva of the mouth; so far, I say, as one may judge from this experiment, I should think that larger doses than ℞ of the solution might be injected in the human adult, if the amount of motor excitement demanded it.

Having now described the method of preparation of the combined solution of the acetates of conia and morphia, and roughly illustrated its physiological action, it remains for me to record details of seven consecutive cases of acute mania, which I placed under treatment by the subcutaneous injection of it, so that we may be enabled to estimate its therapeutic value in such cases.¹

CASE I.—No. in Register, 6329.—Female, aged 41, married; admitted May 31, 1872. First attack; duration being five days on admission, during which time she had been in a fearful state of excitement, had not rested night nor day, and had taken little or nothing except a mouthful of water occasionally. For “some” months previous to this time the patient was depressed in spirits. She had had four children, the youngest of which was nine months of age on her admission, and continued to be nursed by her up till the maniacal accession.

State on Admission.—Wildly excited, shouting and raving, and being violent and destructive; refuses her food; lips parched and dry. Ordered extras of egg and milk, &c., to be given by stomach-pump if necessary, and to be treated by the subcutaneous injection of the solution of conia and morphia, in doses of ℥v or ℥viiss, two or three times a day.

June 2nd.—Decided temporary improvement as to motor excitement, after each injection, but she still talks incoherently, and has to be fed with the stomach-pump at times.

June 4th.—Quieter; tongue and mouth not so dry.

June 5th.—Much quieter both by day and night. Injections have been continued in ℥viiss doses of the solution two or three times a day. She now takes her food herself.

¹ I am indebted to Dr. Crichton Browne, of the West Riding Asylum, for permission to use these cases.

June 7th.—Quiet all night last night, for the first time since admission; tongue clean; much improved. The improvement continued; and on June 15th, as she was quite quiet and rational, took all her food well, and employed herself usefully, injections were stopped.

Sept. 27th.—Having steadily improved since last report, and remained well, she was to-day discharged, recovered.

CASE II.—No. in Register, 6377.—Female, aged 37, married; admitted Sept. 15, 1872.

History.—First attack. She was confined of her seventh child about three weeks before admission, her labour being an easy one. About fourteen days after that, she went to see her mother, who was on the point of death, and, coming home much distressed, is said to have been “taken with fits,” in frequency “many a dozen,” which continued for a day and a night, after which she passed into a state of maniacal excitement, and has so remained, having been for the five days previous to admission “fearfully excited,” and not having slept or taken any food during that time.

State on Admission.—Much excited and very restless, so much so that she had to be placed in a padded room; she shouts and raves in an incoherent manner incessantly, and is very violent and energetic in her movements; she refuses all food, is pale and very weak, and the lips are covered with sordes. Ordered to be fed with egg-flip by means of the stomach-pump, and to be injected with from \mathcal{M}_v to \mathcal{M}_x of the solution, two or three times a day.

Sept. 16th.—Patient is more rational, but still much excited; she was quiet and slept for three hours after the first injection, the change coming on in about ten minutes after the injection, and on awakening she drank some egg and milk herself.

Sept. 17th.—Marked temporary effect for several hours after each injection, and considerable general improvement; takes part of her food herself. After the injection at 10 P.M. last night she went to sleep, and slept almost without interruption until 8 A.M. this morning. Her excitement now is principally confined to garrulousness and slight restlessness. Injection continued twice daily.

Sept. 18th.—Quieter on the whole, and there are glimpses of returning reason.

Sept. 19th.—Injections discontinued last night; improvement continues; she takes all her food herself, and is now fairly rational.

Sept. 22nd.—Got up to-day: discharge from uterus, which was suppressed during the excitement, has now returned; gains strength rapidly, and is now out of danger.

Sept. 24th.—Patient has awakened to her complete senses to-day; is astonished to find herself in an asylum, and remembers nothing of what has taken place during the past few days; she is now a little depressed, but works industriously with her needle.

Sept. 27th.—Is now quite bright and cheerful, and may be termed convalescent. She now remembers having lately been excited and violent, but says she "could not help it."

Oct. 9th.—Improvement is maintained.

Nov. 12th.—Discharged recovered.

CASE III.—No. in Register, 6186.—Male, aged 58; admitted August 30, 1872.

History.—The present, the second attack, commenced about a fortnight before admission, since which time he has been very much excited, and for the last ten days has been in the lunatic wards of the workhouse.

State on Admission.—Wildly excited, shouting, singing, talking incoherently, or moving about incessantly; the expression of his countenance is typically maniacal; he is somewhat pale and weak, and his condition is indicative of rapidly approaching exhaustion.

Treatment and Progress.—Besides plenty of nutrients, in the shape of egg and milk, &c., he was ordered to be subcutaneously injected with ℥v or ℥viiss of the solution, two or three times a day, according to the amount of motor excitement. He slept three hours after the first injection, and steadily improved, so that after six injections it was not, on Sept. 2nd, deemed necessary to continue them: the acute excitement rapidly gave way to a condition of quiet eccentricity, in which the patient attitudinised a little when spoken to. The improvement con-

tinued, and on Sept. 9th he was sent to the convalescent ward to work in the weaving-shed.

Oct. 28th.—Discharged recovered, having remained well and working in the weaving-shed since last report.

CASE IV.—No. in Register, 6194.—Male, aged 30; admitted Sept. 16, 1872, being restless and suspicious, and under the delusion that he was going to be married to the Princess Beatrice.

Sept. 17th.—Patient gets worse, is sleepless, and more restless and excited: an attack of acute mania is fast developing, and it has been considered desirable to quench it at its outset. Ordered the subcutaneous injection of $\mathcal{M}\nu$ or $\mathcal{M}\nu\text{viiss}$ doses of the solution, two or three times a day, as might be deemed necessary.

Sept. 19th.—Marked temporary effect after each injection, which, however, passes away in two or three hours. He was much quieter last night, and not so sleepless as he was.

Sept. 21st.—Patient is much quieter and fairly rational; he does not cling to his delusions with such tenacity as he did, and is disposed to help in the ward. Injections, about twelve of which he had, were now stopped.

Oct. 9th.—A steady improvement has taken place since last report; he has got rid of his delusions, and has been placed in the convalescent ward.

Nov. 14th.—No return of the excitement: has remained well since last report, and will shortly be discharged.

CASE V.—No. in Register, 6196.—Male, aged 22; admitted Sept. 19, 1872, in a state of acute maniacal excitement, with refusal of food, and being restless and noisy at nights. Ordered extra diet and to be subcutaneously injected with $\mathcal{M}\nu$ or $\mathcal{M}\nu\text{viiss}$ of the solution twice or thrice a day.

Sept. 21st.—Patient is somewhat quieter after each injection, but he requires much pressing before he will take his food from the spoon.

Sept. 22nd.—Injections continued: much quieter to-day, and was quiet in bed most of last night.

Sept. 23rd.—Injections discontinued for a time by way of experiment.

Sept. 24th.—Patient has been worse since the injections were discontinued, so that they have been resumed in ℞viiss doses *ter die*.

Sept. 25th.—Quieter again, and takes his food better. The acute excitement passed away in a few days, and the injections were stopped.

Oct. 9th.—He is now quite quiet both by night and day, and takes all his food well; he is not rational, however, and appears to be somewhat demented.

Nov. 14th.—No return of mental excitement, or change in mental condition since last report; but he has gained flesh.

CASE VI.—No. in Register, 6424.—Female, aged 39, married; admitted November 8, 1872.

History.—Patient's father, who was twice in this asylum as a patient, died here as such nine years ago. At the age of eighteen the patient had an attack of melancholia, but was not confined in an asylum. Nine years ago she had her first attack of puerperal mania, but after a stay of eleven months in the West Riding Asylum, she was discharged recovered. The present attack commenced four days before her re-admission, she having been "confined of" her seventh child about nine weeks previously. For the four days previous to admission she was almost entirely without food or sleep, so that when admitted she was quiet and would not speak, and apparently on the verge of exhaustion; she was pale and emaciated, and there was an abscess of the right mamma, which discharged an unhealthy sanguineous pus; there were symptoms of incipient phthisis at the apex of the right lung; the lips and tongue were dry and rough. She was fed with the stomach-pump at once, and placed on extras and stimulants in abundance, including champagne.

Nov. 10th.—Yesterday patient became extremely violent and excited, and it was determined to try the effects of conia and morphia. She had an injection of ℞v of the solution yesterday, but without good effect.

Nov. 11th.—Had three injections yesterday, the dose being increased to ℞viiss, but without effect. To-day she had two injections of ℞x each, but without any effect whatever in

restraining her furious excitement, which has continued without intermission since the 9th instant ; so that, having steadily got weaker, she died this afternoon of exhaustion.¹

In this case, owing to the great debility of the patient and the verging on exhaustion, great care and caution was taken in increasing the dose, the greatest reliance being placed on the liquid nutrients and stimulants which she was made to take ; but, unfortunately, the poor woman died before we reached a dose sufficiently strong to quiet her excitement. This seems to have been a peculiarly unfavourable case from beginning to end, and, to my mind, the unhealthy abscess of the mammae gives rise to a suspicion that there was some blood-poisoning.

CASE VII.—No. in Register, 6273.—Male, aged 40 ; admitted November 12, 1872.

History.—Patient has drunk very heavily for some time past, but, for want of money and other causes, has not drunk at all for the last fortnight. It is his first attack, and it commenced five days ago with delusions and incoherency, since which time he is said to have been wild and restless both by day and night, and to have taken no food.

On admission he was very restless and excited, and stood attitudinising, raving incoherently, in a hoarse whisper, on religious topics, or whistling or shouting aloud. He was put on extras of eggs and milk, and shortly after admission was injected with ℥v of the solution ; he was quiet for several hours after, but at 10 P.M. was restless and noisy, and was again injected with ℥v. He has taken all the nourishment offered him with pressure.

Nov. 13th.—Slept from 11 P.M. to 2.30 A.M. last night, but was restless afterwards.

Nov. 14th.—Injected twice yesterday with ℥v of the solution each time ; he is somewhat quieter on the whole, and takes all his ordinary food and extras well.

Nov. 15th.—Injected once yesterday at 10 P.M. (same dose as before), and was quiet all night, though he was never found

¹ For the notes of this, as well as the two other female cases, I am indebted to the kindness of the medical officers of the female division.

asleep. To-day he is quite quiet and fairly rational, and was ordered up.

Nov. 16th.—Quite quiet and rational, and says he recollects being excited and under the impression that he was fighting battles: he supposes it was the drink that set him wrong.

Nov. 19th.—No return of excitement, goes on well, and may now be called convalescent.

Such, then, are the records of seven consecutive cases of acute mania medically treated by the subcutaneous injection of conia and morphia, and whilst I do not wish to detract from the important rôle which a prompt and liberal dietetic treatment always plays in such cases, yet I think we have reason to suppose, from the facts recorded, that the particular form of medical treatment in these cases contributed in no small degree to the rapid and beneficial results obtained. In only one case did the combined injection fail to have the desired effect; and the fact that an enforced liberal diet, with stimulants, was not, in this case, alone sufficient to induce recovery, gives support to my belief, that neither was it the sole cause of recovery in the other cases. I do not wish to claim for conia, either alone or in combination, more attention than it deserves; but I am decidedly of opinion that it merits a more extended use in cases of acute mania, especially of the kind indicated in the former part of this paper, and as such I can heartily recommend it to my professional brethren. In conclusion, I think it will be scarcely necessary for me to advise great caution in the use of this alkaloid—especially new specimens of it—which has not, so far as I am aware, been before subcutaneously injected in the human subject, and which has hitherto been considered one of the most fatal of alkaloids and unfit for use medicinally. I may add that Messrs. T. and H. Smith, of Duke-street, Edinburgh, and of London, have supplied us with the best specimens of conia, as regards purity and uniformity of strength.

A CLINICAL LECTURE ON MIGRAINE.¹

BY FRANCIS E. ANSTIE, M.D., F.R.C.P.

PART II.—PRINCIPLES OF TREATMENT.

MIGRAINE, then, is a neuralgic affection, usually of the fifth nerve, and belongs essentially to the period of bodily development. Before passing to the question of its treatment, let me dwell earnestly on one or two important additional points of pathological history, which ought to exercise a great influence upon the direction of our efforts. It was first noticed, I believe, by myself, but the observation has been specially confirmed by Eulenburg, that sick-headache most frequently occurs in patients among whose ancestors *epilepsy* had made its appearance; and there are some remarkable clinical features of migraine which, when once they have attracted our attention, immediately recall some of the warning symptoms of epilepsy; namely, the tendency to sigh and yawn, but more especially the strong *shuddering* which is often experienced before the attacks of pain come on. I do not mean to say that a migrainous patient must at all necessarily have had a positively epileptic ancestor; but I think there is high probability that the essential groundwork of the migrainous disposition is an inherited imperfect organization of larger or smaller tracts of the medulla oblongata, and that it is a matter of what we may call accident whether the break-down of nervous health takes the form of migraine, or of epilepsy, or of some other affection (*c.g.* asthma), which is merely allied to them, according as the morbid process affects chiefly the roots of the trigeminus, or the motor tract, or the roots of the pneumo-

¹ Delivered at Westminster Hospital.

gastric, and so on. One of the most striking facts is the frequency with which we find, on careful inquiry, that persons actually suffering from some one of these forms of disease have previously suffered from one or more of the others. The influence which this doctrine of variable neurotic inheritance as a principal factor in the causation of migraine must necessarily exert in our ideas as to prognosis and treatment, must even now be strongly suggested to your minds, and I hope to make it very clear to you.

When you are called, then, to a patient suffering from attacks of migraine, do not allow the outlying phenomena, so to speak, to divert your attention from the central facts as to the pathology of the disease. It is quite possible that the sufferer may complain of the horribly depressing feeling of nausea as much as, or even far more than, of the pain itself; and your impulse may be, not unnaturally, to attempt the direct relief of that sensation by various remedies addressed to the stomach; as, for example, emetics or purgatives to remove some imaginary peccant matter; or, on the other hand, you may be tempted to try a strong cordial stimulant, such as hot and strong brandy and water. The latter might, indeed, give relief; but if so, it would act not on the stomach itself but on the medulla oblongata, upon which alcohol exerts a powerful influence. There are a great number of reasons, however, why it is exceedingly desirable *not* to give alcohol as a direct remedy for nerve-pain; and fortunately in the case of migraine there is no necessity for doing so, for all that alcohol could do can be better done by other things.

Before you decide on using any particular remedy, you should try to make a provisional prognosis, both as to the duration of the existing attack and as to the future nervous history of the patient.

Now, if the sufferer be a young person, somewhere between fifteen and twenty, and the malady has not been going on for more than a few months, you will probably learn that the attacks last for some hours, but that sleep quite puts an end to them, and next day, very likely, the patient scarcely feels as if anything had ailed him, the only reminder being a slight diffuse soreness of the scalp. Of course you wish to relieve his present pain; but is there a likelihood that the curtailment of one attack

will in any degree tend to prevent or delay the periodic recurrence of the affection? Probably it will not do much in this way; for although in neuralgias, which are naturally of more frequent recurrence—returning daily or oftener—we may do most essential service by merely breaking the chain of attacks, we seem to have far less power to effect permanent good, in migraine, by the mere temporary arrest of pain. However, it is well worth trying to cut short the pain. The first thing you should do is to plunge the patient's feet in hot water containing a handful of mustard-flour, and let him inhale the steam that rises; the joint effect of warmth to the feet and of the inhalation of mustard-vapour will often produce drowsiness, and on lying down the patient will feel inclined to sleep. It is admissible to assist this feeling by the administration of a dose of chloral, 20 to 30 grains; but, unfortunately, this cannot always be tolerated by the stomach. Under these circumstances I still advise you to abstain, if possible, from forcing sleep by the use of opiates; and there is one important remedy which, in various forms, is available. The alkaloid caffeine (obtained from coffee), and the substantially identical theine (from tea), and guaranine (in the form of powdered paste made from the fruit of the *Paulinia sorbilis*), are each of them calculated to produce a peculiar effect in relieving the pain and enabling the patient to get repose. In a rough, but not fully satisfactory way, you may sometimes get these effects by administering strong tea or coffee; but much better is the administration of caffeine, in doses of one or two grains by the mouth, or one-half to one grain subcutaneously injected. Lastly, on the recommendation of Dr. Wilks especially, the guarana powder (which is practically a convenient form of impure caffeine) has been brought into fashion in this country,¹ and promises to be exceedingly useful. I gave it, not long since, to a lady suffering from extremely severe migraine complicated with cardiac neuralgia: two of the guarana powders (now sold by all the principal London chemists), given at an hour's interval from each other, gave complete relief to the attack. If we are to believe Riedel and some other German observers, the use of caffeine will not merely give relief to individual attacks, but finally diminish the tendency to their

¹ It has long been used on the Continent.

return; this is a question, however, which requires to be further examined. If chloral be impossible, and caffeine or guarana has failed, we may even yet try one remedy—Indian hemp—before resorting to opiates; it should be given in quarter or half grain doses of a *good* extract, repeated in one hour's time if sleep be not induced. If driven to the employment of opiates, we should give one-sixth of a grain of acetate of morphia by subcutaneous injection.¹

As already said, if we can once induce a tolerably long sleep, the patient will awake free from any acute pain though probably feeling soreness of the scalp. We must then again think of the prognosis for the future nervous state, and consider what can be done to alter the morbid tendency. Male patients are more likely either to lose the attacks altogether on reaching full manhood, or else to find them losing their regular periodicity and also their complication with stomach disturbance, and surviving only in the form of a tendency to ordinary neuralgia of the ophthalmic division of the fifth nerve, occurring at irregular intervals under the provocation of any extraordinary fatigue of either body or mind, especially if this be combined with prolonged *fasting*. Young females, especially if they have menstrual difficulties, and more particularly if they remain unmarried long beyond the natural age for marriage, are more likely than men to keep the migraine in an unchanged form; yet, even in women, there is often a change to a mere tendency to irregularly recurring neuralgia of the fifth, uncomplicated by sickness or nausea, and depending on accidental conditions of fatigue or over-excitement. Sometimes, however, women who begin with migraine, and either remain long unmarried, or, being married, suffer severely from frequent child-bearing and great flooding, develop a fearful form of neuralgia, with very unpleasant and alarming complications. The course of the individual attacks much resembles that of ordinary migraine, and there is often violent and protracted vomiting or retching; but the accesses take place, perhaps, daily (often exactly at one particular hour).

¹ It is unfortunate that caffeine is so insoluble as to require about one drachm of half-and-half water and rectified spirit to dissolve two and a half grains. In hypodermic injections one is thus obliged to inject as much as fifteen to twenty-five minims; no inflammation follows, however.

In some of these cases there is sudden amaurosis; the retina becomes perfectly paralysed, and the patient remains absolutely blind perhaps for twenty-four or forty-eight hours. Nor is there only the danger of this kind of functional disturbance, but, as I have already said, we may get inflammations and other serious lesions of nutrition in the eye; and it is even to be dreaded that we may get iritis, speedily leading to glaucoma and possibly destruction of the eye.

It is thus always a point of the greatest moment to break up the morbid sequence of migrainous attacks, if possible; in order that they may never arrive at such a confirmed and really dangerous stage as this. We may estimate our chances of being able to effect much in this direction, pretty much in proportion to the extent to which the patient's family is free from hereditary neurotic tendencies. The future is very threatening where the family history shows extensive and varied nervous disease, and *vice versâ*. Nothing can be worse, for example, than the probabilities in such a case as the following, which occurred to me in private practice. An exceedingly powerful and well-grown middle-aged man presented himself, suffering from ordinary neuralgia of the first and second divisions of the right fifth nerve; he had suffered from migraine in youth. The family history was appalling: the patient's father and one of his sisters had died insane; one brother had recently had melancholia; another brother drinks heavily: the mother's family is also of very "nervous" type. Here the least serious probability is the recurrence of the neuralgia in a permanent and intractable form: only too likely is the occurrence either of insanity or of epilepsy, or possibly of that paroxysmal form of drinking which is really only a form of inherited insanity.

Be our chance of success great or small, however, we must do our utmost to bring about a radical change in the tendencies of the nervous centres. The first thing to be looked to is the patient's state of general nutrition, and you will often find much here that requires prompt interference. In a great many cases, indeed in the majority, the sufferer from migraine firmly believes that he (or *she*, for migraine is much commonest and most intractable among women) has proved to demonstration the unwholesomeness of all articles of food but some two or three, and of

the general advisability of keeping to a semi-starvation allowance. Especially these people are afraid of taking any form of *fat*; they believe that such things are bilious, and specially likely to aggravate their complaint. Now, nothing can be further from the fact than these notions, in my opinion. You will never make much progress in the cure of the migrainous tendency till you have got your patient to eat and to digest a really good supply of nutritious food: and you will find that fat, so far from being specially objectionable, is particularly useful, as my colleague Dr. Radcliffe first showed. Cod-liver oil is the best thing, if it can be taken, but very often the patient will not or cannot bring himself to touch it at first, so we must begin with very small quantities of Devonshire cream or plain cream, and so get on by degrees to cod-liver oil; and by the time that the patient has come so far as to take the latter to the extent of six or eight drachms daily, you will often notice with surprise how much less fastidious the appetite is with regard to miscellaneous articles of diet. Upon this soon follows a real improvement in the nervous symptoms: the attacks probably become less frequent, losing their rhythmical character, and when they do occur the pain is more tolerable, and seldom culminates in a burst of vomiting.

The effect of cod-liver oil is much aided by iron, as you might expect in cases where there is decided anæmia, the lips and gums being pale and the tongue marked by the teeth: it is best to give the saccharated carbonate in full doses, 10 to 20 grains three times daily. But I do not believe iron is any use in the majority of the cases which are not anæmic.

Migraine shares to some extent the peculiarity of all neuralgias of the first division of the trigeminus to be benefited by quinine:¹ but I beg you not to fall into the common error of thinking that every periodic affection is to be treated with quinine. You may look on the practitioner who flies to quinine on every occasion when a nervous disorder shows some rhythmicality of recurrence as a probably ignorant person. As to migraine, most cases that are curable at all are curable rather by the tonic and dietetic plan above described than by quinine. Arsenic, however, does seem, in some anæmic cases, to act

¹ For neuralgias of other nerves, unless malarial in origin, I seldom employ quinine.

more efficiently than iron both in improving the blood-making process and in lessening the tendency to pain. And on the whole I am inclined, for the future, when quinine does seem indicated, rather to give a prolonged trial to caffeine or guarana, which appear to have a more decided influence than quinine.

Having indicated to you the paramount necessity of improving the general nutrition, it seems almost idle to add that a part of the regimen should be free daily exercise in the open air. You would be very wrong, however, to carry this to the limit of severe fatigue: nothing more strongly predisposes to a relapse than this kind of exhaustion. The question also arises whether, and how much, the brain should be employed in study: this is important, and often very difficult to settle aright. *Exhausting* study is out of the question: but to leave the mind vacant is by no means a good thing, for it leaves the patient (often a sensitive and imaginative person) open to all the disturbing influences of emotion. Now, as regards the influence of emotion, I would beg you to remember that the subjects of migraine are frequently young persons whose organism is but incompletely consolidated, and who are undergoing that marvellous series of changes by which the sexual organs and instincts are developed. Such an epoch assuredly is one of severe peril for the tender and incompletely developed higher nervous centres: as we can see it must be, when we consider the powerful shocks which the keener emotions can give—as evidenced by the palpitating heart, the flushing face, the disturbed respiration, so instantaneously produced, in sensitive persons, by strong hope, or fear, or sexual passion. You cannot keep emotion entirely away from young persons, nor ought you to attempt to do it; but at least you should take care, as far as you have any influence, that they are not encouraged in those more selfish forms of it in which the individual becomes absorbed in the consideration of his own state of mind. False religious excitement, and premature sexual excitement, may be said, in sober earnest, to be twin-poisons of the nervous system of the young: indeed, it is the duty of medical teachers to go a step further, and explicitly declare that there is a most remarkable and intimate connection between the two. Many well-meaning persons were horrified when Mr. Buckle, in his “History of

Civilisation," announced this fact in language which doubtless was exaggerated and unfeelingly harsh : but a fact it is, and without the least sensationalism we may add, a most awful one. If you inquire of those whose lives are spent in tending the insane, they will tell you that emotional religious insanity is, in an immense proportion of cases, attended with heightened sexuality, and the whole field of clinical observation of nervous diseases is full of striking illustrations of the same momentous parallelism. Spiritual rapture, or spiritual woe, of the exaggerated kind, is no purifying influence to the young; it is a *force déchirante*, a demon that rends and tears. The consequences, then, of teaching which appeals to the conscience by any of the more sensational modes are apt to be extremely disastrous in more than one direction: and, as a matter of fact, we meet occasionally with young patients whose minds are a prey at once to the most extravagant religious fanaticism and the most degrading sensuality. I need not say that the young are apt enough to stray, without any special provocation, into sexual faults; but the most frightful peril in which the mind can be placed is that which comes from mingled spiritual exaltation and erotic impulse. It is the bodily side of all this with which we are alone concerned. It is my duty to point out that the shattering influence of such forces seems to fall (next to the brain itself) most heavily upon the medulla oblongata: migraine is one frequent result; others are asthma (from pneumogastric irritation) and convulsions, which may vary in type from the purest "simple epilepsy" through all the grades of hysteric motor explosion.

In cases of migraine which tend to pass into epilepsy (whether from original predisposition or from the incidence of some special force, such as fright, mechanical shock, religious excitement, drink, or sexual indulgence), the ordinary symptoms usually become aggravated before the outbreak of convulsive phenomena. [Dr. Buzzard, whose experience at the National Hospital for the Paralysed and Epileptic is necessarily large, informs me that epileptic patients, in his experience, very commonly say that they had experienced tremendously severe headaches, accompanied with sickness, but *returning day after day*, shortly before the outbreak of the attacks.] For my own

part, I believe it would be found that the daily or very frequent repetition of the headaches would be found to represent a transition stage in a series of processes which begin with true (monthly or bimonthly) migraine, and, if unchecked, ends with epilepsy. Personally, I have observed that when the epileptic affection has become fully developed, the patients have often lost the tendency to headache of severe, or at any rate of neuralgic, type.

Whenever you begin to suspect any tendency towards epilepsy, you will have to act with the greatest promptitude and decision. It is therefore very necessary that you should know what the symptoms are which might inform you of impending danger. I have already hinted that the rapid succession of headaches (still retaining the sick-headache type) is probably always of serious import: and if to this be added brief attacks of that semi-unconsciousness which is often believed by the patient and his friends to be faintness, but is really the *petit mal*, or lesser epileptic attack, then of course you will know that regular epilepsy may at any moment break out. Other more occasional warnings are abnormal sensations of creeping or formication of the skin of one side or one limb, momentary rushing feelings in the head, or the sensation of a breeze or *aura*: but this latter rarely occurs except as the immediate precursor of a fit. The moment that epilepsy either occurs or evidently impends, the patient must be put on a complete and rigid *régime*. He must be at once put on a course of bromide of potassium, in not less quantity than 90 grains daily: and it will be best, especially in young and growing subjects, to accompany this with a fair amount of cod-liver oil. The methodic and careful use of gymnastic exercises,¹ to a degree that will cultivate strength and the power of accurate co-ordination of movements without inducing muscular prostration, should be at once commenced. The utmost vigilance must be exerted to defend the patient from the pre-occupation of the mind with sexual thoughts. His life should be exceedingly regular and quiet; his food as copious as it is at all possible to be made consistently with really effective digestion. His brain is by no means to be allowed to remain entirely unoccupied—a

¹ Such exercises are fully described by Mr. Maclaren in his excellent work on Physical Education.

strictly moderate but regular quantity of intellectual exertion will be beneficial; but every form of painfully disturbing emotion is to be sedulously guarded against. The skin should of course be kept clean and in working order, and when the circulation is vigorous and the power of reaction good, cold tubbing is desirable; but it is a serious mistake to extend this practice to persons of languid circulation, who are only capable of getting up an imperfect or temporary reaction after the bath: here it is simply mischievous. I need hardly add, that any but the most limited use of alcohol is to be absolutely shunned.

In cases where migraine, without taking on any dangerous form, simply becomes converted into an ordinary trigeminal neuralgia without stomach complications, and with no regular periodicity, but coming on with severity on occasion of any great fatigue or shock, we have chiefly to trust to the influence of prolonged tonic regimen, and may indulge a fair hope that as the frame consolidates and grows strong, the affection will gradually wear itself out: such has been the course of events in my own case, and I know many other patients who are now scarcely ever troubled with a bad attack. For women, the entire change of life produced by marriage not unfrequently disperses the last remnants of a migrainous tendency; but, *per contra*, married life has certain dangers which may tend to revive the neuralgic disposition; such as pregnancy and (much more frequently) the depressing influence of severe labours with much flooding, or of lactation. Occasionally, indeed, we meet with an unlucky woman who, being a subject of migraine at the time of marriage, continues to suffer from it throughout the whole period of sexual life, with severe aggravations whenever any physiological crisis takes place: fortunately these cases are comparatively rare. On the whole, migraine may be described as a youthful neuralgia, tending for the most part to extinguish itself after passing through transitional form, but occasionally proving to be only the first symptoms of formidable diseases, such as epilepsy, asthma, angina pectoris, or facial neuralgia of the more violent and persistent type.

[NOTE.—A friend has pointed out to me (what I had certainly quite forgotten) that Sir Thomas Watson, in quite the earlier

editions of his lectures, distinctly classed migraine as a neuralgia. The fact is, however, that although Sir Thomas, in his beautiful chapter on Neuralgia, does say that hemicrania is the "migraine of the French," he describes as such what is only an ordinary trigeminal neuralgia with some stomach complication but certainly does not correspond to the classical descriptions of migraine either by French or by German writers. Authorities in general have identified the idea of migraine with a complaint such as I have now been speaking of: viz. headache of extremely regular periodicity and *long intervals*, attended with nausea and regularly producing vomiting if it is not quickly cut short by sleep, and always put an end to by the latter; always preceded, moreover, by sundry highly peculiar prodromic symptoms.

Let me add, that I never intended to represent myself as having been the first person to suggest that migraine was a neuralgia; various authors had done this during the last twenty years. But I believe no one, before myself, suggested that migraine is a central disease, affecting the medulla oblongata, and having thus an anatomical kinship, as well as hereditary and clinical connections, with an important group of nervous diseases.—F. E. A.]

ON JUNOD'S BOOT AS A REMEDY IN SOME FORMS OF AMENORRHŒA.

BY E. GARRETT-ANDERSON, M.D.

THE difficulty often found in restoring the menstrual flow when it has been suddenly suppressed by cold or nervous shock is sufficiently well known to render it needless to apologise for mentioning a new or unfamiliar remedy. It is notorious that all drugs supposed to be emmenagogue in character are apt to fail when but for the absence of the catamenia the patient would appear healthy. The most trustworthy of the group, iron, probably fails far more often than it succeeds when given in the absence of symptoms of anæmia, and as a rule anæmic symptoms are not present when menstruation has been suddenly and as it were violently suppressed. These are, moreover, precisely the cases in which remedial measures are most urgently needed. In the more common forms of amenorrhœa, where the condition appears to depend upon immaturity, change of air, anæmia, phthisis, or general debility, the suffering directly due to the absence or irregularity of the menstrual loss is generally very slight, and the remedies, when any are needed, can be allowed to act slowly. In the sudden suppression of menstruation from cold or nervous shock, the case is very different. All the symptoms are urgent. The headache is violent, the face is flushed, the eyes congested, the pain in the back severe and the general excitement great. The physician who might treat this condition on the expectant method would probably have few opportunities of doing so twice on the same patient. But the remedies at disposal are lamentably uncertain. Several of the best on the list, such as hot hip and foot baths, mustard

baths, hot whiskey toddy, hot bottles to the feet, pennyroyal tea, and colocynth pills, are usually applied without medical advice. The external application of leeches, though less uncertain than most remedies of its class, often fails. Leeches applied internally, nitrate of silver to the os and cervix uteri, galvanic intra-uterine pessaries, and other direct stimulants to the interior of the uterine cavity, are inadmissible in the case of young unmarried women, for whom even stimulating vaginal injections would be ordered with considerable reluctance.

It is in cases of this kind, where the more ordinary and semi-domestic remedies have failed, where the distress from cerebral congestion is great, and where the suppression of menstruation has been sudden and has appeared to be due to some external or non-constitutional cause, that Junod's apparatus appears to me to promise to be of use, either as a palliative or as a cure. Essentially it is a dry-cupping on a large scale. The leg is enclosed in an air-tight case, and the air exhausted over it. The result is, that for the time the capillaries and vessels of the leg enlarge, more blood remains in the limb, and the circulation in other parts is proportionately relieved. It has the great merits of not interfering with other treatment, of being quite painless and so easily regulated as to be practically harmless, and of being applicable to young girls. I have tried it in two cases, and in each the return of the menstrual flow appeared to be the result.

In both cases the suppression dated from a violent chill during the catamenial period. In the first, the menstrual molimen was not very severe, but there was more or less constant headache and great depression and restlessness. There was no anæmia, and all ordinary remedies had been skilfully and actively applied under medical advice for some months before I saw the patient. The first remedy I tried was faradisation, placing one pole over the lumbar vertebræ, and varying the position of the other over the ovarian and uterine regions. I also extemporised an arrangement by which it was possible to apply the poles directly to the vaginal surface of the cervix uteri without using a speculum. After each application the patient said she felt for some hours as if the loss would shortly begin, but we were always disappointed. My patience failed

after about six or eight trials, and I resolved to see the effect of Junod's boot at the next monthly period. After it had been applied twice, each time for about forty minutes, the flow appeared, and it has since returned regularly. Even after the first application the headache was relieved, and the general aspect of the patient unmistakably improved.

The second case was marked by much greater urgency in the symptoms. The suppression had occurred suddenly two months before I was consulted, and both at the time of the suppression and during the next succeeding period of molimen all the usual remedies had, I was told, been actively applied by the family doctor in the country. When I saw the patient the headache was severe, the skin hot, and the pulse quick and feverish. Very hot sitz baths, with cold lotions to the head, a small blister to the neck, and two leeches on the thighs, relieved the headache, but did not bring on the flow. The next day, faradisation and an ammoniated vaginal injection and more hot baths were tried, without effect. Two days afterwards the headache returned to an intense degree. I then applied Junod's boot; and while it was on, the face became less flushed, the dull, pained, oppressed expression lightened, and the patient declared herself better. The application lasted forty-five minutes. The leg was considerably swollen at its close, but the patient comparatively comfortable. The catamenia appeared the same evening. It is of course impossible to say how much, if any, of these results was due to the cupping process.

It is quite possible that the credit ought to be given to some one of the other remedies, or to all of them, or to none. Where the suffering of the patient is urgent, the desire to remove it naturally outweighs any leaning one would otherwise feel towards making a trustworthy therapeutical experiment.

Reviews.

The West Riding Lunatic Asylum Medical Report. Edited by J. CRICHTON BROWNE, M.D., F.R.S.E. Vol. II. London: Churchill, 1872.

WE congratulate the able and energetic director of the West Riding Asylum on this second number of a series of Reports that is evidently destined to become permanently established. A more entirely meritorious work was certainly never commenced. To utilise the enormous material found in our lunatic asylums for the purposes of systematic scientific inquiry would seem to be a sufficiently obvious duty, yet it is one that has been rarely fulfilled; and in the special matter of therapeutical inquiry—all-important as that subject is—scarcely anything had been done, if we except the excellent researches of Dr. Clouston, and a few others of minor importance. The West Riding Asylum Reports, on the contrary, are in large degree devoted to therapeutical inquiry, and already some very valuable results have been produced. We may also fairly hope that the example of Wakefield will do even larger good by stirring up the officers of other asylums to efforts for the advancement of therapeutics.

Of the very able paper by Dr. Browne himself, on "Cranial Injuries and Mental Diseases," it is not our province to speak, further than to recommend it to the study of the profession. The interesting observations of Dr. G. Thompson on "The Sphygmograph in Epilepsy;" of Dr. W. A. F. Browne on "Impairment of Language, the Result of Cerebral Disease;" of Dr. Aldridge on "Ophthalmoscopic Observations in general Paralysis and after the Administration of certain Toxic Agents;" and the "New Method of determining the Depth of the Grey Matter of the Cerebral Convolutions," described by Dr. H. C. Major, are also beyond our limits. But all the rest of the papers are deserving of study in a therapeutical point of view.

Of the interesting paper, by Dr. J. Wilkie Burman, on "Conia in Subcutaneous Injection," we shall not speak, except to praise it warmly in general terms: because the reader will find a further development of the subject in a paper by Dr. Burman in the present number of the *Practitioner*. The article of Dr. Suther-

land, on "Menstrual Irregularities and Insanity," may be said rather to furnish material for therapeutical reflection than to point out any direct lesson in treatment. The next paper, however, dwells immediately and with much ingenuity and thoughtfulness on questions which are of high practical importance. In recording some researches on "The Effects of Ether and Nitrous Oxide combined," Dr. Samuel Mitchell raises again the whole discussion as to what is the real nature of the effects produced by small doses of those substances which in large quantities are acknowledged to be paralysing narcotics.

Dr. Mitchell's actual experiments seem to show that the combination of ether vapour and laughing-gas gives safe and pleasant anæsthetic results. But these special remarks have led him to general reflections on the subject of stimulation and narcosis; and he has come to certain conclusions, which are important if true, but which we venture to think are not proven. Especially singling out the views expressed in "Stimulants and Narcotics," he attacks the opinion that small doses of narcotics exert truly stimulant effects; and maintains that these supposed stimulant effects are, in fact, only minor degrees of narcotic paralysis. Now, we must note at the outset that Dr. Mitchell has misunderstood Dr. Anstie on an important matter: thinking that the latter reckons mental "exhilaration," "flow of ideas," and the like, as evidences of stimulation, whereas in reality he took much pains to explain that these are early stages of true narcosis, and are perfectly distinct from the phenomena enumerated as those of "genuine stimulation." It is conceded entirely by that author, and we believe that this view has been pretty generally accepted, that vinous excitement and the parallel phenomena under the action of other narcotics are due to the removal of the higher brain-control by commencing paralysis, at any rate of the vaso-motor centres, perhaps of the cortical grey matter. The question of whether such a thing as real stimulation by these medicines be possible, or whether drug-stimulation is not altogether a myth, must be decided by looking to the action of smaller doses, which produce no mental exhilaration, except the mere relief of painful fatigue, no hurry of ideas, no vaso-motor paralysis. Dr. Mitchell maintains that Snow was right in his original theory, that all narcotics act by impeding oxidation within the body. And he further goes on to throw discredit, apparently, on all theories which suppose the possibility of an elective action of particular drugs on particular tissues. We certainly cannot for a moment admit it to be proved that the whole phenomena even of undoubted narcotism are caused by impeded oxidation; and as for the denial of the elective action of drugs, we are even more hopelessly in opposition to Dr. Mitchell's views. We are ready to grant, of course, that

practitioners did, and still do, very often immensely overrate possibilities of accurately affecting particular organs with drugs: but that such particular actions do nevertheless take place an experienced therapist can hardly doubt, even though he cannot always formulate the process to his own mind. If Dr. Mitchell (to take a few examples) were to deliberately say that digitalis has not a distinct and peculiar effect on the heart, or that belladonna has not a special action (as in eneuresis) upon the muscles of the bladder, or nitrite of amyl upon the vaso-motor nerves, we should be inclined to say that he has enjoyed very little either of experience or of study and reflection. However, we are not ungrateful to him for his spirited effort to throw more light upon questions which are still, it must be owned, sadly dark.

Another very important paper is by Dr. Clifford Allbutt, on "The Electric Treatment of the Insane," a subject that much needed candid and persevering investigation. The observations were made with a Muirhead (Daniell) battery of 100 cells. The current from 5—20 cells was sent directly through the head, and occasionally applied to the sympathetic in the neck. The cases selected for treatment belonged to the categories of mania, brain-wasting, dementia, and melancholia. In one case of brain-wasting the treatment was ineffective, or possibly harmful: and the same result was observed in three cases of melancholia. A more encouraging effect took place in two cases in which the current through the head and then through the sympathetics was used: real good appeared to be done; but the most positive and marked benefit was conferred in cases of acute primary dementia. The hypochondriacal variety of melancholia seemed to get worse under the current. Dr. Allbutt says that Dr. Major and himself both observed that among these insane patients there were very remarkable differences of sensibility to the current, whether applied to the head or elsewhere. One of the most positive physical effects was the restoration of warmth where the temperature was low. In catalepsy, the rigidity was always removed, or lessened in a remarkable manner. Altogether these observations, which are fully and carefully reported, form an exceedingly valuable beginning to a scientific knowledge of the value of electricity in mental diseases, which we certainly have not hitherto possessed.

The last paper we must notice is that of Dr. Courtenay on "Opium in Melancholia." This is a very effective reply to the doubts and objections which have recently been raised against the treatment in question. As an answer to Dr. Clouston's statements, so far as they apply to melancholia, it seems to us complete. We fancy the author is wrong in supposing that Dr. Maudsley extends to opium in melancholia the objections which

he has recently so strongly stated to its use in insanity generally. At any rate the readers of the *Practitioner* will remember an exceedingly interesting article by him in the number for January 1869, in which he speaks most highly of opium as a remedy for melancholia. For our own part, we entertain no doubt that opium is as strikingly useful in melancholia as it used to be disastrously mischievous when pushed in large and repeated doses to quiet the excitement of mania, according to old-fashioned principles.

[Reviews of Cooper's Surgical Dictionary, Erichson's Science and Art of Surgery, and Gant's Science and Practice of Surgery, with other reviews, will appear in our January number.—ED. *Practitioner*.]

Clinic of the Month.

Phosphorus in Neuralgia.—Dr. S. M. Bradley, of Manchester, in a letter to the *Lancet*, observes that phosphorus is a remedy little, if at all, employed by the regular practitioner in cases of neuralgia, though, he believes, it is frequently exhibited by the homœopath. A case, however, came under Dr. Bradley's notice some time since, which so strikingly illustrated its value as to induce him to record it. A gentleman who had for years suffered acute and frequently recurring paroxysms of neuralgia of the chest walls, applied to him for advice. He found that he had been a regular round of London and provincial doctors, and that every plan and remedy had been tried, from quinine to Pulvermacher's chains and heroic doses of arsenic. Suffice it to say that Dr. Bradley failed as signally as his predecessors in affording him anything but temporary relief, and, under the circumstances, could not repine at his seeking aid from the homœopath. His surprise, however, was considerable in finding that ease for which the patient had so long sought, and sought in vain, was procured in a very brief space of time by this change of doctors. Dr. Bradley lost no time in ascertaining the remedy which had worked so speedy and, as it proved in the sequel, so permanent a cure, and found that it consisted of the so-called mother tincture of phosphorus, of which he was ordered to take five drops on the advent of an attack, and repeat them as occasion required. This tincture of phosphorus is a solution of phosphorus in ether, which dissolves about 1 per cent., so that each dose—contains about one-twentieth of a grain of phosphorus scarcely homœopathic according to old-fashioned notions; *mais cela va sans dire*. Not only was the pain relieved, but the frequency of the attacks was lessened, until from suffering a seizure two or three times a week, as he had for some years, he has now been entirely free for more than four months. Since the occurrence of this case Dr. Bradley has frequently employed this preparation of phosphorus, and has often found it of signal service in curing neuralgia; especially, it has appeared, in those

subjects who add to a highly nervous temperament some cause of nervous waste: so that it may be considered probable that the neuralgia has indeed in these cases been, as Romberg styled it, "the cry of the hungry nerve for blood," or, rather, for its own special pabulum in the blood, and that the phosphorus has directly supplied this want. Dr. Bradley has also employed pills of phosphorus melted in suet and coated with gelatine, a preparation recommended by Squire when phosphorus is indicated; but it has not been found that they possess any advantage over phosphoric ether, while they possess the disadvantage of being difficult of preparation, and the universal pillular drawback of doubtfulness of destination, whether of absorption into the blood, or of excretion by the bowels. (*Lancet*, Nov. 9, 1872.)

Ætiology and Treatment of Epilepsy.—A writer in the *Medical Times and Gazette* classes convulsions and epileptic seizures, as to causation, in such a grouping as—First, due to direct toxic agents (animal, mineral, and vegetable poisons, diseased condition of blood, as met with in the exanthemata, syphilis, gout, and the like, including articles of diet or drinks when taken in excessive quantities, or when themselves impure or deleterious). Secondly, all causes of imperfect aëration of blood, whether heart or lung disease, or mechanical or other causes which either impede or prevent the access of air or blood to the lungs, or the due oxygenation of blood in the tissues. Thirdly, losses of blood or other fluids, causing direct general or local anæmia, and local congestions or obstructions which may induce anæmia of the nervous centres. Fourthly, local causes of irritation, whether acting as direct excitants of nervous and muscular action, or by reflex action through or upon nerve-centres (worms, teething, tumours, &c.). Fifthly, other causes not classified here—mental or moral shocks, perhaps.

In regard to the treatment of these several forms, if the cause be malaria, quinine, barks of various kinds, arsenic, bromides, and the like may be administered, and in a minor degree opium, mineral acids, and many of the sulphates, besides that of berberin. When the cause is the poison of gout or syphilis—colchicum, veratria, squill, digitalis may be prescribed for the former, and mercurials and iodide of potassium for the latter: few cases of epilepsy are so curable as those depending upon syphilis. When lead or mercury is the cause, the iodides and perhaps the sulphates and potassium chlorate, with alkaline sulphides and baths, come to our aid. This writer thinks it extremely probable that most of the mineral tonics, silver nitrate, zinc salts, and the like, as well as the ferruginous remedies which for many years were so fashionable for the relief or cure of epilepsy, owed, or owe their success to their properties as

antiseptics or disinfectants, by virtue of their powers of combining with albuminous matters and of resisting decomposition; or perhaps, like quinine, to their property of limiting the ameboid movements of white blood-corpuscles or of other animal cells or cellules. If this be admitted, the difficulty of reconciling the apparently opposite qualities of a host of drugs will vanish. Creosote and carbolic acid, ammonia, alcohol, ether, and chloroform will all claim to be ranked in this category. Under the second class, or those remedies which may aid where lung or heart disease—impeding free aëration of blood—is present, another long list of remedial measures and drugs will find a place. Change of air, of diet, and of occupation, remedies which relieve congestion, as purgatives and bleeding, diuretics and diaphoretics, and such as act upon the nerves of the heart or lungs—notably digitalis, hyoseyamus, the bromides, ethers, and chloroform, and such means as cod-liver oil, or phosphorus and its compounds—may each and all be included in this category. In the third class, all the means of arresting hæmorrhage, and all the means of making fresh blood or improving the old—hæmatines and the like—and even rest, are deservedly placed. Allusion, and only allusion, may be made to the forceps of the dentist. The chlorodyne of Dr. Collis Brown, oil of male fern, kameela, kousso, turpentine, santanine, and all other anthelmintics, sedatives, and whatever may either remove or soothe the local peripheral irritations which disturb the nervous centres, must be placed in the fourth class. The fifth class of the causes of epilepsy, as it is vague and wide enough to include all the unknown causes, should also include all those wonderful and inert specifics which succeed only in the hands of their own inventors: such are the tractors of a Perkins, the marvels of mesmerism, hypnotism, and a hundred things as generally useful as condurango or the uric acid of a boar. (*Med. Times and Gazette*, Oct. 26 and Nov. 9, 1872.)

On the Treatment of Enteric or Typhoid Fever.—Dr. Little remarks that the standard text-books on the treatment of this disease are not sufficiently explicit, and in fact all is not stated that can be done in cases of typhoid fever. The opinions that he now expresses are founded on treatment of his own cases. Next to early confinement to bed, which perhaps more than anything else lessens the severity and risk of the fever, he ranks the rigid exclusion of animal broths and jellies from the food, as tending to keep the disease mild. In this point he finds himself at variance with the text-books, in which such articles as beef-tea and Liebig's essence of meat are recommended. Milk should be the chief article of diet in enteric fever. Thirsty

patients sometimes object to its mawkish taste, and in that case ice should be added, and a little lime-water in cases where it returns curdled. Junket or renneted milk, given before it has separated into whey, and curd, rice milk, custard, baked custard in small quantities, rusks and hot milk, and blanc-mange, generally afford sufficiently varied ways of giving milk. Freshly made chicken jelly is less liable than beef-tea to increase the abdominal symptoms, in those cases where milk even with lime-water disagrees; but Dr. Little finds that this is a very rare occurrence, and when encountered is usually in a person chronically dyspeptic. For years he has made the administration of two or three cups of really good tea or coffee, between day-break and two in the afternoon, a regular part of the treatment in every case of fever, unless there was in the state of the nervous system some evident contra-indication. This he recommends in consequence of the well-known observations of Dr. Parkes on the effect of coffee in increasing the elimination of urea in fever; and Dr. Little finds that both it and tea lessen drowsiness and prostration, and increase the secretion of urine: once or twice in the day they may be given, poured upon a well-whisked egg, and thereby an additional means of nourishing the patient is obtained. Dr. Little considers that alcoholic stimulants in any quantity are seldom needed. Cold baths he thinks serviceable: three, or at most four, may be given in the twenty-four hours. In severe cases he has used them with great benefit, where cooing and wheezing râles exist in the chest, and where deficiency in the percussion-resonance posteriorly and muco-crepitis indicated pleural stasis in the lungs, but not when there was hæmorrhage from the bowels, or such pain as to justify the fear that peritonitis existed. When there is slight chilliness in the extremities after a bath, and shivering, this indicates that it should not be a prolonged one, but does not forbid its use. Twice Dr. Little has considered it unsafe to continue the baths—once because a marked shivering followed, and once because the patient was alarmed by it. In cases of the disease running a mild course, it is not necessary to have more than one bath in the day, at the height of the usual evening paroxysm of fever. By a dietary such as has been described, and the systematic employment of baths, the severity and danger of enteric fever may be greatly diminished, and the occurrence of any of the serious accidents incidental to the complaint rendered very rare, but the period of duration is not shortened. Besides these means, Dr. Little has found others beneficial under certain conditions. When during the first eight days the face is flushed, and there is headache, a high temperature, and a thickly coated tongue, and when the evacuations, three or four in the twenty-four hours, are neither very large nor very liquid, a dose of calomel, from four to six

grains, perceptibly lessens the heaviness of the fever. He has sometimes given the calomel a second time after an interval of a day or two, but never oftener. In enteric fever it is not uncommon to find a patient lying on his back, perceptibly impeded in his breathing, his abdomen tumid and projecting, but not markedly tender; and on inquiry it will be found either that the bowels have not acted for twelve hours, or that, though the stools are frequent, only a very little faecal matter with wind passes each time. In these cases much relief may be obtained by giving a draught containing two drachms of castor-oil with one or two of turpentine. Poultices and fomentations he has not found useful. By keeping patients rigidly to the diet mentioned, it is not found necessary to give medicines to check looseness of the bowels: when it is necessary to interfere, the most useful remedy is a pill containing one-sixth of a grain of carbolic acid, one-sixth of a grain of opium, and three grains of bismuth. Another remedy is sulphuric acid. Hæmorrhage from the bowels is rare when milk diet and cold baths are employed; when it occurs, gallic acid, a scruple every second or third hour, and turpentine, were the remedies upon which he relied. Since ergotin has been shown to possess the power of arresting hæmorrhage, administered hypodermically, Dr. Little has tried it in one case successfully. There is a group of nervous phenomena sometimes present in typhoid fever, for which the remedy is a full dose of quinine. For delirium and wakefulness with severe headache, cutting the hair and leeches are the remedies. Nausea and persistent retching may be relieved by an emetic of ipecacuanha or ice, or a draught containing ten grains of bicarbonate of soda, ten grains of carbonate of bismuth, and four minims of prussic acid. Scantiness of urine requires dry cupping of the loins, and the internal use of the salts of potash and spirit of nitrous ether. Indications of pulmonary congestion, which are sufficiently common in enteric fever, are best relieved by a turpentine stupe. (*Dublin Journal of Medical Science*, No. 5, 1872.)

Treatment of Inveterate Ague.—Mr. Blower, of Bedford, remarks that specific remedies such as quinine and arsenic sometimes fail to effect a cure in cases of ague. Neither large doses nor long-continued administration of these remedies produce any beneficial action, and the disease continues its course uninterruptedly, deranging the structure of the spleen and liver, and deteriorating the general health. The paroxysms become habitual, and they occur regularly at well-defined periods. The time of their advent is well known, and they are expected with positive certainty; and should they not take place at the usual hour, the patients would be astonished, if not dis-

appointed. This establishment of a habit constitutes a formidable obstacle to the cure of the malady. To break this habit is therefore the great object in the treatment of this condition, for when it is broken the fits cease to recur, and only their effects upon the system remain to be combated. This object may be effected in various ways. Thus, by throwing the patient into a state of unconsciousness at the time when the paroxysm is expected, its occurrence may be prevented. In two cases which came under Mr. Blower's notice, where the ague had been of long continuance, and had resisted every attempt to cure, the habit was broken and the disease stopped by the administration of large doses of opium three hours before the times when the fits were expected. The patients were thrown into a profound sleep, and they slept over the hours when the paroxysms should have come on. These did not take place, and they never recurred afterwards. Another mode of effecting this purpose is by occupying the mind of the patient, so as to withdraw his attention from his expected fit until the time for its occurrence has passed by. A man who had been subject to fits of ague, which occurred every other day at noon for a considerable time, was one day, as he was watching the clock for the time when he should be attacked, suddenly called out of doors, and kept closely occupied in an interesting conversation for a short time. Whilst he was away the hand of the clock was put on half an hour, and when he returned into the house he looked at the clock and said, "Bless me, it's half-past twelve, and I have not had my ague." Nor did he have it any more. Also by making a strong impression on the nervous system, the habit may be broken and the paroxysms stopped. There is an anecdote related of a celebrated Lord Chief Justice which illustrates this point. In his youth he had been very gay; and once when he had been drinking with some dissolute companions at an inn, he found that they could not raise money enough between them to pay the reckoning. Looking about to see if he could discover any means of extricating himself and his friends from the dilemma in which they were placed, he observed the daughter of the landlady looking very ill, and he inquired what was the matter with her. On being told that she had ague, he said that he could cure her. He then scribbled something on a piece of paper, rolled it up into a ball, and gave it to the mother, telling her to hang it about her daughter's neck, and she would not have ague any more. The mother did as she was directed, and the girl was cured. Many years after, when he was settled down into a grave judge, an old woman was tried before him for witchcraft. The alleged witchcraft consisted in hanging a ball around the necks of persons afflicted with ague, and in this manner curing them. The ball was produced in court, and the judge on examining it

discovered that it was the identical ball which he had given to the old woman many years before in discharge of her bill, and which had not only cured her daughter, but also all others on whom it had been tried. Before the epidemic of ague in 1827-9, a lady at Bedford cured many patients by giving to them spiders made into pills with flour and water, and the beneficial operation of this disgusting remedy was doubtless owing to its action on the minds of its sufferers. There is a singular tale told of the influence of mental impressions in the cure of this disease. A man who had had ague for a long time, and had become so reduced by it that his life was despaired of, was advised to make his will. One of his bequests was—"I give and bequeath unto Mr. —, the parson of this parish, these plaguey fits of the ague." This legacy so tickled his fancy that he burst out into a loud and long-continued fit of laughter. From that time the ague left him. The clergyman, upon being told of the bequest, was highly offended, but the next day he was seized with ague, and it was a long time before he could get rid of it. (*Lancet*, Nov. 2, 1872.)

A New Method of treating Ulcers.—Mr. Philip Cowen states that successful as many of the various plans of treatment of ulcer are, still too often they are very tedious in operation; very often the result is not at all commensurate with the care bestowed; and not unfrequently the ulcer will not heal at all. Rest in bed, various lotions and ointments, support by strapping and bandages, cold poultices, side incisions, blisters, irritants or caustics, internal remedies, all occasionally succeed; but too often the case becomes alike wearying to the surgeon and to the patient. It appeared to him, from whatever cause arising, whether constitutional or local, an ulcer could be considered as a local asthenia of the skin and parts beneath—a local weakness and loss of plasticity, a brittleness, where softness, elasticity, and pliancy, yet strength, should exist; a local tendency to degeneration and death. Whether the cause was from venous varicosity, or blood-pressure from other causes, as indolence or disease of the liver or heart, or from general decay of the tissues from intemperance or other enfeebling vices, or from senile decay, or want of food, or general feeble constitution, or from scrofula or syphilis—from any of these causes the ultimate result is, in an ulcer, the weakening and giving way of structures whose normal condition is to act as covering and protecting sheaths. In several of these cases the skin is specially weakened, in others it gives way through pressure; and in all cases skin of the legs, from its depending, weight-bearing position, suffers the most. Knowing as we do that an ulcer has power of absorbing matters applied to its surface (anyone can try the

experiment with black-wash to a moderately sized ulcer for a few days, when mercurial salivation will begin), Mr. Cowen availed himself of this excellent property by applying, locally, matters having nutritive powers, so that the skin might be nourished locally at the weakened and degenerate spots, to enable the skin to take up such materials as would nourish its weakness and convert its brittle state into a plastic and healing one. The natural secretion of an ulcer, the so-called laudable pus, may it not be a nourishing juice specially thrown out to feed the young growing granulations? If so, can we not add to this by artificial manuring? Such is the theory; and to carry it out Mr. Cowen made the following mixture of glutinous ingredients:—Flour, four ounces; powder of acacia, one ounce; powder of tragacanth, half an ounce; one egg; chalk, two drachms; cold water, one pint. These were all mixed together and placed on the fire in a saucepan. Just as it began to boil, or after boiling a minute, it was removed and allowed to cool. If found too thick, it was made thinner by adding a little boiling water, and stirring so that it was the thickness of ordinary paste, thin enough to be spread over the ulcer by means of a little brush, yet so thick that it would remain on the ulcer. Probably such a mixture is not necessary; either the flour-paste alone, or the acacia, might be sufficient; but he prefers it as it stands. The patient, provided with a pot and brush, thickly paints the ulcer all over three or four times daily, and covers the ulcer with a thin piece of soft rag. No other treatment, nor washing, is had recourse to, except to remove the rag. To keep the materials sweet, only sufficient is poured into the patient's pot for the day (pot and brush washed daily). The flour, acacia, tragacanth, and the egg, supply the ulcer with all the necessary materials for its repair; the chalk has a further good effect by supplying lime. (*Lancet*, Nov. 16, 1872.)

Extracts from British and Foreign Journals.

Therapeutic Employment of Electricity in Essential Paralysis of Children.—Dr. Bouchut, of the Hospital for *Enfants Malades* in Paris, made the following remarks in the opening address of the course on clinical medicine for the present year. By the side of paralyzes of cerebral or spinal origin, there are in infants certain myogenic paralyzes, rheumatic in their origin, and having their primitive seat in the muscular fibre itself. The evidence of this is, that amongst eleven autopsies of cases of so-called infantile paralysis recently published by different authors, there are four where no disease of the brain was found, nor any in the spinal cord or nerves. As to the seven observations in which there was spinal sclerosis of the anterior and lateral columns of the cord, and sometimes microscopic softenings of the anterior grey cornu, there is no proof that these lesions were not the effect of the abolition of function, and granulo-fatty atrophy of the paralysed muscles. In point of fact, there is always in the rheumatic paralysis of adults an atrophy of the nerves distributed to the muscles attacked. Thus such atrophy is always seen in the nerves of the face after facial hemiplegia caused by cold. In like manner we see that a traumatic lesion of the eye produces sometimes alteration of the optic nerve which extends to the optic thalami, from whence it descends to the opposite eye, compromising its vision. Dyspepsia frequently produces, by ascending irritation of the nerves of the stomach, a lesion of the brain which results in insanity, enteritis, and dysentery ; and diseases of the bladder produce in the same way an alteration of the spinal cord which displays itself by paraplegia. Simple angina and diphtheritis frequently induce, by extension to and irritation of the pharyngeal nerves, a lesion of the brain and of the cord which terminates in amaurosis, and an ascending paraplegia which reaches the thorax and produces asphyxia, &c.

If the disease of an organ can awaken no sympathy and have no irradiation, it may, by a process of ascending neuropathy, attack the brain and the cord, and give rise to secondary lesions. It is consequently not impossible that a myositis of the inferior extremities with granulo-fatty degenerations in an infant may

produce an extensive neuritis affecting the spinal cord. This seems to follow from the fact that in certain cases of paralysis with spinal lesion, the nerves of the paralysed muscles become granular and fatty, and atrophied; whilst they are healthy in subjects where the cord has remained intact.

Additional evidence that the paralysis with fatty degeneration of infancy may originate in the muscles, and not in the spinal cord, is that in a limb attacked with infantile paralysis there are muscles which remain healthy, which would not occur if this paralysis with fatty degeneration were the consequence of a spinal disease causing the suppression of motor excitation of the nerve-roots from whence the principal nerve of the member emanates.

Thus M. Bouchut states that he has seen an infant affected with fatty paralysis of the left arm. The deltoid, the pectorals, the supra- and infra-scapulars, the brachialis, and the triceps were atrophied, non-contractile, and the deltoid had so far disappeared that the humerus could be dislocated in all directions to such an extent that the head of this bone was separated two centimetres from the acromion, so that the finger could be pressed in between the two. Nevertheless, the fore-arm and hand were not paralysed, the wrist could be moved in every direction, and the fingers opened and closed with ordinary vigour, above: the trapezius and splenius were intact, and could powerfully elevate the shoulder. Here was a case of partial paralysis of the upper extremity. How is it possible, he asks, to explain this paralysis of the middle part of a limb by a lesion of the spinal cord suppressing the excito-motor power of the nerves issuing from it? There is only one local lesion of the muscles which can account for a paralysis localised in the middle segment of a limb, and this local lesion is the granulo-fatty myositis. In some instances indeed a part only of a large muscle like the deltoid is affected. There are, then, local muscular paralyses or myogenic paralyses due to a primary alteration in the muscles. The rapidity of the progress of the fatty degeneration is sometimes very great. The contractility of a muscle may disappear in a single night, and in two or three days evident atrophy of the paralysed muscle has already taken place. How could this occur as a consequence of spinal lesion, and in what instance has lesion of the cord followed by paralysis produced atrophy of the muscles in a few days? It really never occurs.

A final proof of the existence of atrophic paralysis of a rheumatismal and granulo-fatty character in the muscles of an infant is found in the success of treatment applied exclusively to the muscle in maintaining its nutrition and recovering its motor power. Thus, if from the second or third day after the paralysis has occurred the capillary circulation is rendered more active

and the nutrition of the paralysed muscles stimulated by *continuous currents*, movement is recovered little by little. Here the action is quite local; neither the nerves nor the spinal cord is excited, there has been no attempt to provoke contractions of the fibrils, and no other object has been in view than to maintain the nutritive hyperæmia of the muscles. In this way such cases may be cured when, by inaction or temporising, time may be lost and the disease become incurable. The following is one of the cases he reports:—Ferdinand B——, eight years of age, very lightly clad, played for some hours in February in a cold wind in the garden of a friend. He returned home, dined as usual, and went to bed somewhat later than usual. He slept well, but the following morning on awaking he experienced a pain in his left shoulder, and was unable to move his arm, which was paralysed. He could bend the fore-arm, and move the wrist and fingers in every direction. He could bring the elbow forwards, backwards, and upwards. But he could not raise the arm to the head: in a word, he had motor paralysis of the deltoid, and to some extent of the pectoral and trapezius. In addition he had some pain on pressure being made upon the muscles affected, and from the third day evident atrophy of the deltoid set in. The anterior and posterior fibres did not respond to electrical excitation, but a few of the centre ones still gave feeble contractions. The same conditions were present in the pectoralis major. The sensibility of the skin was normal, and he had no febrile symptoms. No other indication of disease was present. Anxious about his patient, M. Bouchut obtained a consultation with Nélaton, Broca, Sée, and Gosselin, who established the diagnosis of an attack of paralysis accompanied by fatty degeneration chiefly affecting the deltoid. There was some difference of opinion in regard to the treatment, but none in regard to the diagnosis. Apprehensive of an incurable atrophy of the deltoid, like those which he had frequently seen, M. Bouchut was unwilling to wait till the muscular atrophy had made further progress and had become completely fatty. Not believing that spinal lesion would produce such a paralysis, and seeing no other disease in this child than a muscular lesion, he determined at once to act on the muscular tissue. On the hypothesis of a cerebro-spinal lesion it would have been proper to wait, for it is imprudent to apply electricity at the outset of acute disease of the spinal cord, for fear it should aggravate the symptoms. From the third day M. Chéron, to whose care M. Bouchut committed the child, applied continuous currents of electricity either every day or every other day. These *séances*, combined with vapour baths, in the course of a month led to the recovery of a feeble amount of contractility in the whole of the deltoid and in the pectoralis major. By the fortieth day the

movements of the shoulder-joint were restored to such an extent that the arm could be raised to an angle of 15° . Still later on they became more easy, and ultimately, at the end of the fourth month, the child was perfectly restored to health. At the expiration of three years a slight atrophy of the deltoid was alone perceptible. Apart from this difference, the two upper limbs had precisely the same vigour; and so this child, treated with appropriate means from the outside by muscular stimulants, escaped an incurable deformity. M. Bouchut employs a cheap apparatus, only costing six or eight francs, composed of four small Daniell's cells, modified by Trouvé and Callaud. The current is so feeble that it may be applied for ten or twelve hours without inconvenience. (*Bulletin Général de Thérapeutique*, tome lxxxiii., Aug. 15, 1872.)

On the Percussion of the Spleen.—In an essay read before the St. Petersburg Medical Society, Dr. Eichwald maintained that, as a general rule, the fingers were superior to every instrument for the purposes of percussion. Nevertheless, that there were cases in which it was necessary to define the limits of deeply-seated organs, and in these it was necessary to percuss with considerable force, and it was not always wise to forego the advantages offered by the mallet and plessimeter. For this purpose Eichwald recommends the hard gum plessimeter made by Seitz, in Giessen, at the broader end of which is a split. Near the margin of the organ, drawing the plessimeter constantly onwards, he strikes successively the two portions. As soon as there is a difference in sound, the fissure marks the position of the margin required to be discovered. According to Eichwald it is even possible to determine the line of the highest part of the liver, though Luschka is of a contrary opinion. The upper part of the spleen lies in the hollow of the diaphragm, and is covered by the inferior border of the left lung. Its lower border, which is in immediate contact with the wall of the thorax, under normal conditions does not reach to the arches of the ribs, because it rests upon the ligamentum costo-colicum, which is inserted into the upper border of the eleventh rib, and thus it cannot elongate unless it be greatly increased in weight. This lower border, after the position of the inferior margin of the lungs has been determined, is easily differentiated from the adjoining stomach and colon. When this has been made out, the upper border covered by the lungs must be investigated by means of strong percussion. Under ordinary circumstances the spleen reaches as high as the ninth rib, and often, according to Schuster, even to the eighth. Anteriorly the normal dulness is bounded by a line which runs from the anterior extremity of the eleventh rib to the nipple (the costo-mammillary line).

Posteriorly the dulness belonging to the spleen is not distinguishable from that of the kidney. When the spleen is enlarged and the abdominal parietes are relaxed and the intestines empty, the dulness extends chiefly forwards and downwards, but, with full intestines and light abdominal parietes, upwards, seldom higher than to the fifth rib. In the healthy man the spleen has a length of from 4 to 5 inches, a breadth of from 3 to 4 inches, and a thickness of from 1 to $1\frac{1}{2}$ inches. It weighs about 4,000 grains. A spleen that is more than 12 centimetres (5 inches) long and 8 centimetres ($3\frac{1}{2}$ inches) broad, must be considered to be enlarged. The position of the patient is of importance in determining the limits of the spleen by percussion. The sitting posture is unfavourable, because the lower ribs then approximate one another till they touch or even overlap; and at the same time the adjoining parts resound on percussion, obscuring the local resonance. Decubitus on the right side has the same disadvantages. Eichwald, like Schuster, recommends a sort of special position; the patient lying flat on the pelvis, but twisting the upper part of the body to the right, so that he rests upon the right shoulder. In this way the left lower ribs are separated from each other, the subjacent organs are also drawn asunder, and the resonance upon percussion is distinct. Individual and accidental difficulties are presented by senile sclerosis of the ribs, repletion of the stomach and of the transverse colon, which often render percussion of the spleen impossible, and emphysema of the lungs. The descending colon is rarely troublesome, for this almost always forms a tympanically resonant band two inches in breadth between the posterior limits of the spleen and the vertebral column. (*Petersburger Zeitschrift*, Band v. Heft 4; and *Der praktische Arzt*, 1872, p. 151.)

The Treatment of Boils by Alcohol.—M. Nélaton has for more than twenty years prescribed the use of alcohol for the prevention of these smaller abscesses which are so common amongst young people, and which so seriously impair the beauty of the face. It appears that this treatment is now extending, as some interesting details are to be found in a communication of M. Simon de Forges, of Sainte-Fourtaïne, which appeared in the *Revue de Thérapeutique*. In speaking of boils, small outward abscesses, and other inflammations of the epidermis and of the derma, he observes that as soon as the characteristic circular redness appears on any part of the body, whatever may be its size, with a point rising in the middle, making it a greyish white, a thimbleful of camphorated alcohol should be poured into a saucer; the palm of the hand should be wetted with it, and this should be rubbed with gentle friction over the affected place. The fingers should be again steeped, and the

friction continued as often as eight or ten times every half minute. The place should be well dried, and before covering it up, a little camphorated olive oil should be applied to prevent the evaporation of the fluid. (*Journal de Médecine*, October 1872.)

The Action of Narceine.—The sedative qualities of opium are due to a greater mixture of alkaloids than any other remedial means. Nevertheless, whilst some of these, as morphia and narceine, produce sleep, others, as narcotine and thebain, produce convulsions and cramps. It is obviously, therefore, expedient that when it is desired to produce sleep, morphia and narceine should be administered in a pure condition, apart from those alkaloids which, by their exciting action on the nervous system, would tend to prevent sleep. The best experimental investigations of late times demonstrate that narceine is better adapted for the purpose of inducing sleep than morphia; that it is entirely free from all exciting action, and acts as a pure narcotic. This has been demonstrated by poisoning animals with narceine, when they die in sleep without any indication of convulsions. Similar experiments, though of course not carried so far, have been made on man, and have shown that the sleep produced by narceine differs from that of morphia in its deep and quiet character, and the unexcitability of the body to loud noises or other stimuli.

The phenomena of paralysis and mental debility, remarked as a rule after the use of morphia, are entirely absent after narceine, as are also the headache, feeling of indisposition, and nausea which sometimes lead patients to object to that remedy.

Eulenburg was the first who instituted experiments with narceine. It dissolves in 375 parts of water at 17°C., and it may be used both as an internal medicine and hypodermically. It is best given, on account of its high price, by the latter method, for whilst when administered internally from one-sixth to half of a grain is required, only one-eighth to one-quarter of a grain is requisite when subcutaneously injected. Since the larger of these doses have no deleterious secondary action, they may be increased if occasion require; indeed it may be given in the same quantities as morphia itself.

Narceine, besides its general narcotic properties, may also be advantageously employed as a local sedative, producing not only temporary alleviation of pain in many cases of neuralgia of peripheral origin, but also permanent and complete recovery.

Narceine is a highly nitrogenous compound, found only in opium, and forms colourless columnar crystals. It has a bitter pungent taste; it melts at 92°C., and unites with difficulty with acids. With several mineral acids it forms combinations that turn blue on the addition of water, become colourless on the

addition of more water, but again become blue on the addition of chloride of calcium. (*Aerzt. Literaturblatt*, No. 9, 1872.)

Strychnine Poisoning treated with Hydrate of Chloral.—(By S. A. Turner, M.D., attending surgeon, Grand River Agency, D.T.; reported by D. D. Crothers, M.D., Albany, N.Y.)—The purpose of this communication is to add another to the rapidly accumulating cases illustrating the power of the chloral hydrate over the convulsions produced by strychnine.

The patient in this case was a Sioux Indian, apparently aged 45, and revelling in the luxury of a plurality of squaws, one of whom, in a fit of affectionate jealousy of his attentions to the other, administered to him a quantity of strychnine upon food which she had prepared for his special enjoyment. Dr. Turner was sent for, distance four miles, and saw the injured man at 12 A.M. All that he could learn through my interpreter, an "intelligent contraband," was that the sun was shining, and must have been "pretty high," when the food was taken; and that the "warrior," finding the food unpleasantly bitter, swallowed but a single mouthful, soon after which the sense of sight became much impaired, and convulsions ensued, spasm of the posterior muscles of the trunk predominating, with opisthotonos strongly marked.

When we first saw him the intervals between the recurrence of the spasm varied from thirty to sixty seconds. There was spasm of all the muscles of voluntary motion. No food had been taken since the day before, except the morsel containing the poison.

He administered 30 grains of chloral. In thirty minutes the convulsions ceased, and did not recur until the expiration of thirty minutes more. Then there was a mild spasm, soon followed by a severe one. Gave 15 grains of chloral, and the patient soon became tranquil, and remained so for thirty or forty minutes, when, another severe spasm occurring, he administered 30 grains of the remedy.

For a few minutes the patient was tranquil, with power of vision nearly normal. The patient rested quietly for three hours, evincing considerable drowsiness at times. A slight convulsion ensued, when he gave another 30-grain dose of the remedy.

There was no return of spasm. The patient slept all night, and when seen next day complained only of intense muscular soreness. Recovery satisfactory. (*Med. and Surgical Reporter*, June 15, 1872.)

The Subcutaneous Injection of Extract of Secale Cornutum upon Fibro-myoma of the Uterus.—Prof. Hildebrandt, of Königsburg, has recently been experimenting on this

agent in certain diseases of the uterus. His first case was that of a woman aged 33, who was suffering from a fibro-myoma of the uterus, and who had profuse menstruation (though this occurred but once a month), followed by very abundant fluor albus, and accompanied by severe uterine pains and spasms, with a feeling of weight in the abdomen. These symptoms she had experienced for three years. The tumour had attained such a magnitude that the uterus appeared equal in size to that of a woman in the twenty-eighth week of pregnancy. Many remedies had already been tried, as iodine, both internally and externally, ergot, perchloride of iron and lead. Compressed sponges and laminaria had been used to dilate the orifice of the uterus, and to render the tumour accessible to surgical interference; but these measures had to be given up on account of the great tenderness of the os uteri.

Prof. Hildebrandt now determined to try the effects of the daily subcutaneous injection for fifteen days, into the walls of the abdomen, of three grains of the watery extract of ergot in glycerine. Menstruation was induced, but the quantity was materially diminished. The tumour now began to diminish, and five weeks after the commencement of the injection had entirely disappeared. The sound could be introduced to its normal extent, the fluor albus and pain were abolished, and menstruation was rendered regular. After this singularly successful result, Prof. Hildebrandt adopted it in nine other cases of uterine fibroids, and found it successful in all but two. In one of these the use of the remedy had to be given up, owing to toxic effects, and in the other on account of the extreme pain accompanying the subcutaneous injection. He recommends the employment of the ergot in solution in glycerine rather than, as Langenbeck proposes, in alcohol, on account of the less pain occasioned by the former solution, and he finds the most convenient part to be the vicinity of the umbilicus rather than the inferior part of the abdomen, on account of its lower degree of sensibility. To prevent the escape of the fluid from the wound after it has been injected, he recommends that the orifice should be touched with collodion, and a pad of cotton-wool applied. (*Berliner Wochenschrift*, No. 25, 1872, and *Der praktische Arzt*, No. 8, 1872.)

Notes and Queries.

DEPARTMENT OF ANALYSIS AND INVENTIONS.

TISY'S PREPARATIONS OF IRON.—Prepared by M. Tisy, Pharmacien à Paris. Sole agents for England, Messrs. Corbyn & Co.

These preparations consist of mixtures of a protosalt of iron, the lactate, with either carbonate of sodium, iodide of potassium, bromide of potassium, or both these latter salts, and starch, or dried mucilage of starch. The mixtures are enclosed in capsules made of gum and sugar, so as to form somewhat large-sized pills. The capsules, being entirely closed, preserve the contents from contact with the air, and thus prevent oxidation, while at the same time they are very readily soluble in the gastric juice, and thus allow the salts contained in them to be dissolved. One of the objects aimed at, viz. the non-oxidation of the iron salts, is very perfectly attained in these preparations. It is, however, very problematical whether the other, the formation of the desired compound within the stomach, is equally fulfilled; but this is probably of minor importance so long as the necessary elements, in a soluble condition, are introduced. We must, however, protest against the use M. Tisy makes of the term "nascent." Many elements in their nascent state, *i.e.* at the moment of their liberation from a compound, have remarkable properties, not possessed by the free element. Effects may therefore be produced by the nascent element which are unattainable by means of the same element when once liberated. The formation of a compound, however, offers no analogy whatever to this, and the term, as employed by M. Tisy, becomes misleading, since peculiar properties are expected to be possessed by the preparations to which they can lay no real claim.

The analytical details in regard to the various preparations are given in the table annexed. An examination of this table will show, amongst other things—

1. That in three at least out of the four preparations the quantities of the dose contained in one capsule vary considerably, and thus one of the chief requirements of a medicine, constancy of dose, is not complied with.

2. That the dose contained in any one capsule is very small. Thus the dose of the B. P., *pilula ferri iodidi*, containing 18 and 21 per cent. of iron and iodine respectively, is from 3 to 8 grains. To administer the equivalent of this would require from $2\frac{1}{2}$ to 7 of the capsules for the iron and from 6 to 17 for the iodine; while to obtain the ordinary dose of carbonate of iron from 10 to 40 of the corresponding capsules would have to be taken.

3. The amount of iodine and bromine in no case corresponds to the amount of iron present.

Notwithstanding the above drawbacks, we believe, however, that these preparations will prove valuable additions to our stock of medicinal preparations, whenever the difficulties of their manufacture have been more perfectly surmounted, since they evidently permit the employment and storage of very readily oxidisable drugs without detriment to their medicinal qualities.

The capsules are sold in well-corked cylinders of blue glass, holding from 80 to 90 capsules, at a price of 4 fr. per glass.

Nature of Preparation.	Mean weight of full capsule.	Weight ¹ of contents of capsule.			Weight of active agent.		
		Mean.	Max.	Min.	Iron.	Iodine.	Bromine.
Fer Bromuré . .	4.44	1.66	1.69	1.00	0.15	—	0.120
Fer Ioduré . .	4.41	1.42	1.96	1.28	0.12	0.100	—
Fer Iodo-Bromuré	5.05	1.10	1.19	1.07	0.10	0.042	0.054
Fer Carbonaté . .	2.82	1.25	1.50	1.04	0.20 ²	—	—

¹ All weights are given in grains.

² Corresponding to 0.37 grs. proto-carbonate.

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